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A MODEL FOR THE DEMONSTRATION OF FACIAL EXPRESSION

By E. G. BORING and E. B. TITCHENER

Ever since Wundt brought apparatus from the laboratory to his desk in the lecture-room it has been clear that a general course in psychology should, so far as possilbe, be made demonstrational. The best demonstrations, as Wundt saw, are those that invite the members of the audience to observe for themselves; the worst are those that merely display and describe some instrument of research. Even these worst, however, are very much better than nothing; they enable the lecturer to give a concrete account of procedure; and now and again they persuade some mechanically-minded student to enter the laboratory.

For the introductory courses given at Cornell University we have worked out an extensive set of demonstrational pieces, some of the older of which have been described in this JOURNAL.[1] The various topics handled in these courses present varying degrees of difficulty to the demonstrator; and we have found Feeling to be the most difficult of all. In the lectures on Simple Feeling we have been obliged, so far, to content ourselves with two demonstrations: a demonstration of the method of impression, by way of paired comparisons of colors, and a demonstration of the method of expression, by way of the pneumograph.[2] Emotion has long baffled us. We tried, first, to construct a series of charts by means of drawings and photographs of postural and facial expression; but the series remained woefully incomplete, and the demonstration also consumed too much time. We next thought of an artist's manikin; but we found none on the market whose articulation met our psychological requirements; and in any case the impossibility of facial change

[1] E. B. Titchener, Class Experiments and Demonstration Apparatus, this JOURNAL, xiv., 1903, 439ff.

[2] Neither of these arrangements has been described.

makes the lay-figure an unsatisfactory performer.[3] Finally, we decided on an attempt to realise, in demonstrational form, Piderit's 'geometry of expression,'—not by way of the plaster heads which he himself suggests, but more simply by way of an 'articulated' profile.[4]

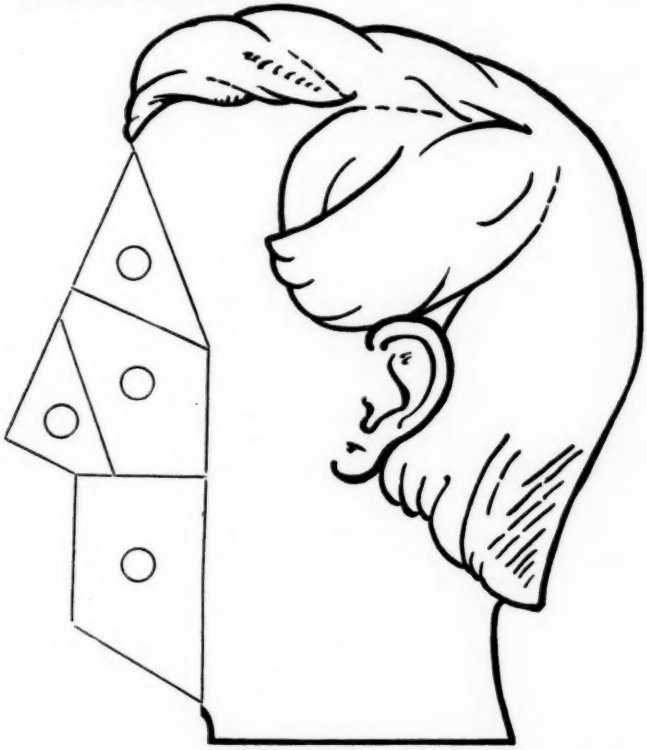

FIG. I

The model consists of the profile of a head, about 18 in. high, with various brows, eyes, noses and mouths, that can be

[3]Cf. E. Toulouse et H. Piéron, *Technique de psychologie expérimentale*, ii, 1911, 85 ff. (measure of memory of attitudes and expressions). We have not altogether given up the idea of a demonstrational manikin or human phantom.

[4]Th. Piderit, *Mimik und Physiognomik*, 1867, 1886, 1909. The third edition was reprinted in 1919. The full title of the first edition was *Wissen-*

fitted into place after the manner of a puzzle picture. The basic outline is shown in Fig. I., and the pieces in Figg. II. and III. The 18 in. head is satisfactory in a room seating about 250; for larger audiences the model should be larger.

The original model at Cornell is made of white cardboard with the outlines drawn in India ink. On the back of every piece is glued a rectangular piece of wood which fits into a rectangular socket in the square wooden base upon which the head is mounted. The base-board is supported on a tripod, inclining slightly backward from the vertical. This arrangement works perfectly: the parts stay in place and, being independent, remain in position even when pieces below them are removed. The contrast between the white cardboard and the black lines greatly assists the visibility of the picture. The parts need, however, to be kept carefully, in order that the cardboard shall not curl up or get soiled or become battered at the corners.

A wooden model was later constructed for the Clark Laboratory with the intention of obtaining a more durable demonstration that could easily be transported for lecture purposes. This model was made of several plies of white wood. The head was raised from the base-board the thickness of one ply (⅛ in.). A large socket ¼ in. deep of the shape shown in Fig. I. was made by cutting out the central ply (¼in.), and into this socket four blocks, ¼ in. thick, for the four features, exactly fitted, as shown by the outlines in Fig. I. The profile is carried on every piece by an additional ply (⅛ in.), which is glued to the block and which projects, in different amounts for the different expressions, beyond the outer line of the block. The pieces are removed by pushing with a finger through a hole at the back of the model; these holes are shown as circles in Fig. I. The Clark model has been less satisfactory than the Cornell model because sufficient attention was not given to balancing the plies one against another, and the model has consequently warped. This model also shows less contrast than does the white cardboard. In it the white wood was filled with white shellac and the outlines were drawn in India ink directly upon the wood. The resultant pieces are not all of the same color, because of differences in the wood, and the yellowish surface gives less visibility than the white. A better wooden model could be constructed, if more attention were given to the principles of multi-ply construction, and if the surface of the wood were painted white.

The outlines of the model are all copied directly from Piderit-except for a few minor modifications and slight changes neces, sary to secure perfect registration in any combination of pieces.[5]

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schaftliches System der Mimik und Physiognomik. It was preceded in 1858 by a smaller work, *Grundzüge der Mimik und Physiognomik*, which we have not seen. There is also a French translation of the second edition (1888): *La mimique et la physiognomie.* The dates for Piderit, 1858 and 1867, are interesting when taken in connection with the dates for Darwin, 1859 and 1873. In the first edition of Piderit there is no mention of Darwin; the second edition discusses specifically *The Expression of Emotions in Man and Animals* (1873) and refers repeatedly to Darwin.

[5]We wish to acknowledge the assistance of Mr. W. S. Hulin of the Harvard Psychological Laboratory in the preparation of the drawings for Figg. I.-III. of this article.

The principle, upon which the expression of the emotions is demonstrated by the model, consists in showing that given meanings, often biologically explicable, can be attached to given simple expressions, and that the complication of expressions by mere addition leads to a total expression which is intelligible as a complex of the correlated meanings. The approach in lecture is best made by way of Darwin's principles of remnants of originally serviceable habits and of antithesis.[6] It is necessary to add to these principles the corollary that contradictory expressions do not always lead to paradoxical expressions, since a total expression may properly and simultaneously mean contraries. Thus contempt involves at once both attention and inattention (*cf.* Fig. VII., 23). One may even introduce here the notion of the Freudian conflict.

When the expressions to be compounded pertain differentially to the brow, eye, nose and mouth, the addition is obtained simply by combination of the different pieces of the model. When two expressions pertain to the same feature it is necessary to have a new piece showing the combination of the two expressions (*e.g.*, the combination of the open mouth, Fig. II., 7, with the bitter mouth, Fig. II., 4, as in Fig. II., 8).

A very large number of features could be made. The present list does not exhaust Piderit, and Piderit does not exhaust the possibilities. Some indication of promising additions will be given below.

On the other hand, the present set permits a very great number of possibilities. There are here 360 possible combinations, and most of these turn out to be sensible, largely because the combination of contraries often gives a natural expression for a conflicting emotion. Some expressions are, however, unnatural or impossible (*cf.* Fig. V., 12, and possibly Fig. VI., 15).

One begins in lecturing by listing (preferably on a blackboard) the simple expressions with their corresponding meanings. For the pieces of the present model these correlations are as follows.

Mouths

The mouths are shown in Fig. II., and will be referred to as M1, M2, etc. The mouth, according to Wundt,[7] is fundamental to the expression of the emotions, because taste qualities, like sweet and bitter, are naturally correlated with pleasantness and unpleasantness and hence with acceptance and rejection, and are further correlated physiologically with regions of the tongue, which is most sensitive to sweet at the tip and most sensitive to bitter at the root.

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[6] C. Darwin, *Expression of the Emotions in Man and Animals*, 1873, 27-65.

[7] W. Wundt, *Physiologische Psychologie*, iii., 1911, 264 ff.

M1. Normal mouth = passivity. Piderit does not give this inexpressive mouth. It is necessary, however, as the neutral point of departure for other expressions (*cf.* Fig. IV., 1).

M2. Sweet mouth = pleasantness. The lips are pressed together and drawn in and the tongue is pressed forward, so as to keep the sweet substance in the mouth and to get the greatest effect from it. M2 is the basis of the pleasant expressions.

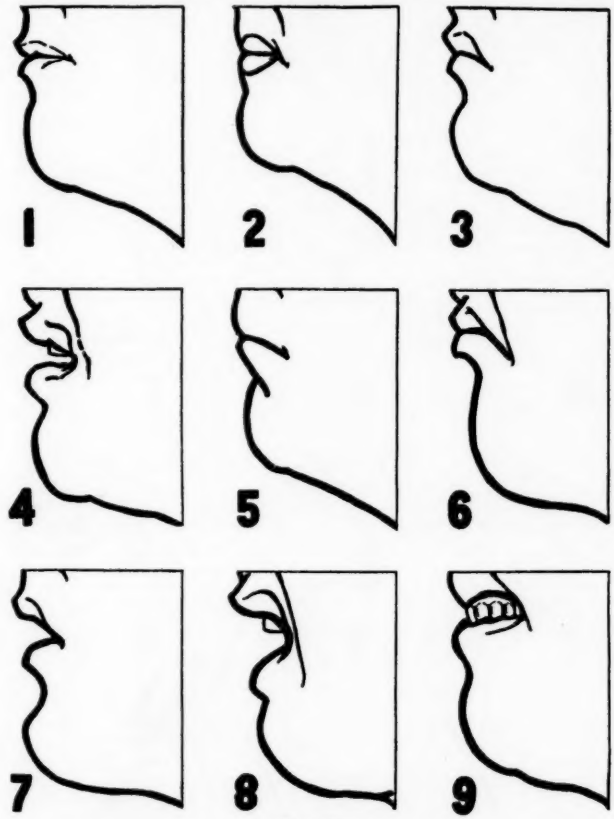

FIG. II

M3. Bitter mouth = unpleasantness. The root of the tongue, most sensitive to bitter, is drawn down to avoid stimulation by the bitter substance, thus changing the contour of the neck. The lips are thrust out in incipient spewing (the pout). M3 is the basis of the unpleasant expressions.

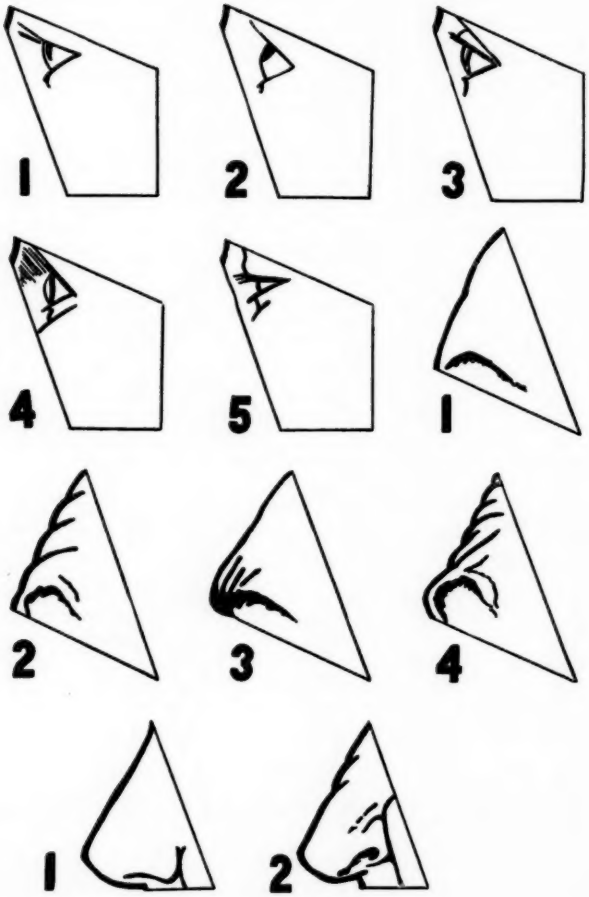

FIG. III

M4. Extremely bitter mouth = great unpleasantness. This expression is simply an exaggeration of M3. The tongue is drawn further down, the lips are protruded further, and the mouth is a little open: a more extreme attitude of rejection.

M5. Set mouth = stubbornness. *Der verbissene Zug* (Piderit) is only imperfectly translated by the word "set." The biological explanation is not obvious, unless one turns to Darwin's third principle, the spread of nervous energy.[8] The lips are protruded, but, otherwise than in the bitter expression, are pressed tightly together; thus the corners are drawn down. The expression fundamentally means determination, but the compressed lips give it also the meaning of rejection. Thus it is correlated with obstinacy or stubbornness.

M6. Bitter and set mouth = unpleasant stubbornness. This expression is not new, but is a combination of M4 and M5, which can not be made by combining two pieces since the two expressions pertain to the same feature.

M7. Open mouth = attention. The biological explanation is presumably better breathing rather than alimentary readiness. The present outline has been changed from Piderit by enlarging the chin to what M1 would be when opened. The open mouth in Piderit is especially vacuous because of the receding chin; but in general attention belongs to the eyes and brow and is not shown well by the mouth alone (*cf.* Fig. IV., 6).

M8. Bitter and open mouth = unpleasant attention. This expression is a combination of M4 and M7. It is part of horror (*cf.* Fig. VII., 19).

M9. Clenched teeth = rage. The mouth is meant to show the bared canines of the snarl or sneer.[9] It occurs in anger (*cf.* Fig. VII., 20), and is too extreme an expression to appear alone.

The sour mouth of Wundt might be added.[10] Piderit does not give it, and Wundt, copying Piderit's sweet and bitter mouths in profile, found it necessary to draw the sour mouth in full-face.

There are also the possibilities of adding smiles of various degrees, and the laughing and weeping mouths. Piderit draws these in full-face, and it may be that they could not be shown adequately in profile. It is not impossible that the present model could advantageously be supplemented by a second in full-face. If laughter and weeping are to be represented, it would be necessary to add corresponding eyes, brows and noses.

Eyes

The eyes are shown in Fig. III., and will be referred to as E1, E2, etc.

E1. Normal eye = passivity. This eye is the neutral point of departure for other expressions.

E2. Raised eye = exaltation. The expression is symbolic and as such is distinctively human. It tends to imply 'introversion,' since reality is mostly upon the ground.

E3. Wide open eye = attention. Human attention is primarily visual. Ordinarily the brow is raised also (B2). This expression is fundamental to the meaning of 'extraversion.'

E4. Very wide open eye = great attention. This eye is simply more extreme than E3, and is always combined with the raised brow, B2 or B4. Properly it belongs only to the brow that is both raised and contracted (B4), since it is the contraction of the brow that produces the shading above the eye, copied from Piderit's original drawing of horror (*cf.* Fig. VII., 19).

E5. Lowered lid = inattention. This expression is basal to 'introversion' or to voluntary inattention. It can be referred to Darwin's prin-

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[8] Darwin, *op. cit.*, 66-82.
[9] *Ibid.*, 249ff.
[10] Wundt, *loc. cit.*

FIG. IV

No.	M	N	E	B	No.	M	N	E	B
1.	1	1	1	1	2.	2	1	1	1
3.	3	1	1	1	4.	4	1	1	1
5.	5	1	1	1	6.	7	1	1	1

ciple of antithesis (E5 is the opposite of E3), or to some more fundamental biological principle, allied to the conditioned reflex, which explains why animals, including, it is said, the ostrich, refuse to look facts in the face. Most facts that have to be met are predominantly visual, and are thus partly removable by removing the visual perception.

The wide open eye (E3), especially when combined with the raised brow (B2), may mean memory as well as attention. It is not at once obvious why attention and memory should be similar. As an invariable correlate of memory Piderit lists the moving eye (not easily incorporated in a model), thus anticipating the experimental finding of Perky forty-three years later.[11] The moving eye, the wide open eye, and the raised brow are all the imitative kinaesthesis of remembering.

Brows

The brows or foreheads of Fig. III. are referred to as B1, B2, etc.

The brow goes properly with the eye and should ordinarily be treated with it. Normally the wide open eye and the raised brow are part of the same expression. They may, however, contradict each other in a complex expression (*cf.* Fig. VII., 22-24).

B1. Normal brow = passivity: the neutral point of departure for the other expressions.

B2. Raised brow (horizontal furrows) = attention. The expression belongs naturally to the wide open eye (E3, *q.v.*), but may give the meaning of attention apart from the eye.

B3. Contracted brow (vertical furrows) = unpleasantness and thought. The assignment by Piderit of two such apparently different meanings to the same expression seems odd at first, although one notes that active thought of the contracted brow is usually supposed to be unpleasant. The common term, on the side of meaning, seems to be 'introversion.' The unpleasant is avoided; thought is subjective. In both the reality of visual perception is avoided (*cf.* E5).

B4. Raised and contracted brow (horizontal and vertical furrows) = unpleasant attention. There is nothing incompatible in the meanings of B2 and B3 to prevent their combination, except that, since the raised brow means attention ('extraversion'), the contracted brow with it can not mean thought ('introversion') and must mean unpleasantness. There is here, nevertheless, a genuine conflict positively expressed. The organism meets an unpleasant situation either by attending to it (raised brow) or by withdrawing from it (contracted brow), and the two contrary tendencies are simultaneously registered, as, for example, in horror (*cf.* Fig. VII., 19), where the organism tends to withdraw and yet is compelled to attend.

Noses

There are two noses, N1 and N2, in Fig. III.

N1. Normal nose = passivity. The nose is the least mobile part of the face and ordinarily can not be innervated alone. N1 is therefore the nose of all mild expressions. Only in extreme emotions need N2 be substituted for it.

N2. Dilated nostrils = attention and unpleasantness. It seems probable that the biological ground for this expression lies in olfactory attention and sniffing, although the dilatation of the nostrils in anger must be related to the deeper breathing which is part of the total picture of preparation for attack. The assertion in olfactory attention of the unpleasantness of the object presents the same puzzle which confronts us when we try to explain the taboo upon smells in conversation and literature.

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[11] C. W. Perky, this JOURNAL, 21, 1910, 422-452.

FIG. V

No.	M	N	E	B
7.	1	1	3	2
9.	1	1	1	3
11.	1	1	4	4

No.	M	N	E	B
8.	1	1	5	1
10.	1	1	5	3
12.	1	2	1	1

Henning indicates that most smells are potentially pleasant; and yet when the nose of Fig. VII., 24, asserts, in contempt or disgust, "How you smell," the expression inevitably takes on an uncomplimentary meaning.[12] (*Cf.* the sniff of contempt, thumbing the nose, etc.)

It has been suggested that an intermediate nose between N1 and N2 would be desirable in the model. If one were, however, to multiply the number of pieces in the model by the discrete degrees of expression, the demonstration would become impossibly complicated.

If laughter and weeping were added, two more noses would be needed. According to Piderit the nose is almost the only *differentia* between these two expressions. It is slightly dilated in weeping and spread broadly in laughter.

The foregoing expressions may be recapitulated under their meanings as follows.

Meanings

Pleasantness is shown by the sweet mouth (M2).

Unpleasantness is shown by the bitter and extremely bitter mouths (M3, M4), which are attitudes of rejection; by the contracted brow (B3), which is an attitude of withdrawal; and by the attentive nose (N2),—which may mean that unpleasant odors are the odors most often attended to by most persons. Since thought is also a withdrawal, it is likewise indicated by the contracted brow.

Attention is the most important attitude of the organism, and is shown by the open mouth (M7), the wide open eye (E3, E4), the raised brow that belongs to the wide open eye (B2), and the dilated nostrils (N2).

Inattention is indicated by the lowered eye-lid (E5), which may be enhanced by the contracted brow (B3).

Stubbornness is the set mouth (M5). *Rage* is the clenched teeth (M9). *Exaltation* is the raised eye (E2).

Total Expressions

Of the 360 possible combinations of the pieces, 24 are shown in Figg. IV-VII. These cases are selected as showing well the range of the model and the principles of demonstration. The cuts for the figures were photographed from diagrams made by pasting together parts from prints of Figg. I-III. The profiles of Figg. IV-VII. are therefore not redrawn, but are actually obtained by mechanical addition. In the model itself the cracks between the pieces (*cf.* Fig. I.) show, but they are not nearly so prominent as the lines of the profiles.

The following numbers refer to the total expressions of Figg. IV-VII. The words describing the expressions are intended merely to be suggestive; such words do not admit of rigorous definition. The expression is really defined by its analysis.

1. *Passive.* The relaxed inexpressive face. M1+N1+E1+B1.

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[12] H. Henning, *Der Geruch*, 1916, 177-183. As Henning divides thee smell prism there would be only one-half to one-third as many unpleasant odors as pleasant. Actually the proportion of examples that he gives is even more in favor of the pleasant odors. On the fallacy of the olfactory taboo, see E. B. Titchener, *Text book of Psychology*, 1910, 116; *Beginner's Psychology*, 1915, 50 f.

No.	M	N	E	B
13.	2	1	2	1
15.	2	1	1	3
17.	7	1	3	2

No.	M	N	E	B
14.	2	1	1	2
16.	3	1	1	3
18.	4	1	3	2

FIG. VI

2. *Pleased.* Pleasantness (sweet mouth). M2+N1+E1+B1. A very mild expression; the sweet mouth is not a smile.

3. *Displeased.* Unpleasantness (bitter mouth). M3+N1+E1+B1.

4. *Very displeased.* Great unpleasantness (extremely bitter mouth). M4+N1+E1+B1. The expression is not striking, because the contracted brow should be added, as in no. 16.

5. *Stubborn.* Refusal and determination (set mouth). M5+N1+E1+B1.

6. *Attentive.* Acceptance (open mouth). M7+N1+E1+B1. Oral attention is, however, much less expressive than visual attention; *cf.* nos. 7 and 17.

7. *Quizzical.* Attention (wide open eye and raised brow). M1+N1+E3+B2.

8. *Inattentive.* Withdrawal or 'introversion' (lowered eye-lid). M1+N1+E5+B1.

9. *Displeased, thoughtful.* Withdrawal or 'introversion' (contracted brow). M1+N1+E1+B3.

10. *Deeply thoughtful.* Still greater withdrawal; combination of nos. 8 and 9 (lowered lid and contracted brow). M1+N1+E5+B3.

11. *Dismayed.* Displeased (contracted brow) attention (wide open eye and raised brow), showing a conflict between the tendencies for withdrawal (contracted brow) and acceptance (raised brow and wide open eye). M1+N1+E4+B4.

[12. *Unpleasant olfactory attention.* M1+N2+E1+B1. This expression is practically impossible, and is presented here to emphasize the point that the nose is a feature that, unlike the mouth, eye and brow, contributes to expression only in combination with changes in other features.]

13. *Reverential.* Pleasant (sweet mouth) exaltation (raised eye). M2+N1+E2+B1.

14. *Affable.* Pleasant (sweet mouth) attention (raised brow). M2+N1+E1+B2.

If the factor of attention in no. 14 is enhanced by the wide open eye, we get Piderit's drawing for *sweet memories*: M2+N1+E3+B2. A drawing is inadequate, however, since the eye should rove.

15. *Pleasant unpleasantness*, a contradiction between the sweeth mouth and the contracted brow. M2+N1+E1+B3. *Os* differ as to the meaningfulness of this expression. It is anatomically possible, and one *O* characterized it as the passage from pleasant to unpleasant thought. The case illustrates the difficulty of finding any combinations that are meaningless simply because they are contradictory.

16. *Displeased and thoughtful.* Unpleasantness (bitter mouth, contracted brow) and withdrawal (contracted brow). M3+N1+E1+B3.

17. *Bewildered.* Very great attention (open mouth, wide open eye, and raised brow). M7+N1+E3+B2.

18. *Amazed.* Unpleasantness (bitter mouth) and great attention (wide open eye and raised brow). M4+N1+E3+B2.

19. *Horrified.* Great unpleasantness (bitter mouth, dilated nostrils, contracted brow) and great attention (open mouth, dilated nostrils, wide open eye, and raised brow). Really a meaningful contradiction, since it is an attentive withdrawal (brow raised and contracted). M8+N2+E4+B4.

Anger is based upon the clenched teeth, M9. When the clenched teeth, are combined with the wide open eye and the raised brow, one gets a pleasant anger: M9+N1+E3+B2. The contracted brow, however, gives an unpleasant anger: M9+N1+E1+B3. Great anger, as the model shows it, is unpleasant, though the brow is both raised and contracted: M9+N1+E4+B4. In very great anger the nose is also dilated, and this expression is shown as no. 20.

No.	M	N	E	B
19.	8	2	4	4
21.	5	1	3	2
23.	6	1	5	2

No.	M	N	E	B
20.	9	2	4	4
22.	5	1	5	2
24.	6	2	5	2

FIG. VII

20. *Raging.* Unpleasantness (dilated nostrils, contracted brow), great attention (dilated nostrils, wide open eye, raised brow) and aggression (clenched teeth). M9+N2+E4+B4.

21. *Disapproving.* Resistant (set mouth) attention (wide open eye, raised brow). M5+N1+E3+B2.

No. 21 has been described as "Mr. Grundy." A related expression is that of the "extreme Puritan," where the set and bitter mouth is combined with the raised eye: M6+N1+E2+B1. The mouth gives obstinate disapproval, and the eye a withdrawal from the mundane.

22. *Disdainful.* Refusal (set mouth), inattention (lowered lid) and attention (raised brow). M5+N1+E5+B2. The contradiction between the raised brow and the lowered lid is meaningful. Disdain is both attentive and inattentive; it points to an object and denies interest in it.

Disdain is the first member of a related group of expressions. The next are *amazed disapproval*, M6+N1+E3+B2, where the conflict of disdain is partly eliminated and the bitter mouth is introduced, and *mild contempt*, M6+N1+E5+B1, where there are the bitter stubborn mouth and the attentive eye. There is still a trace of conflict here, for the mouth refuses while the eye attends. Then follow nos. 23 and 24.

23. *Contemptuous.* Unpleasantness (bitter mouth), rejection (set mouth), withdrawal (lowered lid), and attention (raised brow). M6+N1+E5+B2. This figure develops the meaningful contradiction of disdain, no. 22.

24. *Disgusted.* An enhancement of no. 23 by the addition of the dilated nostrils, which heighten the meaning of attention and point unpleasantness to the object. M6+N2+E5+B2.

In view of the fact that many contradictions are meaningful, it is of interest to note that there are some besides no. 12 and possibly no. 15 which appear to be senseless. No *O* has yet read sense into the following combinations: the attentive (open) mouth and the inattentive eye (lowered lid), M7+N1+E5+B1; the aggressive (clenched) mouth and the reverential (raised) eye, M9+N1+E2+B1; the sweet mouth and the turned up (dilated) nose, M2+N2+E1+B1.

To ensure a smooth-running demonstration, we arrange the 20 movable pieces as follows. A squared wooden rod is laid horizontally in V-shaped rests on the lecture-desk, below and in front of the tripod which carries the model; and the pieces are 'hooked' over this rod by the rectangular bits of wood glued to their backs. The audience thus sees the whole series of mouths, eyes, brows and noses, laid out in that order; and the lecturer, who has made his guiding marks upon the backs of the pieces, removes and replaces them as need arises. Some such arrangement is necessary, to prevent confusion and consequent loss of time; and the demonstration loses nothing by the show of dexterity with which the lecturer manipulates his facial keyboard.

IMAGERY IN THE WAKING AND DROWSY STATES[1]

By Hulsey Cason, University of Kansas

I. General Statement

The claim has been made by certain writers that the most prominent varieties of imagery present in the waking state are not the most prominent varieties in the drowsy state. Other writers have maintained that the particular kind of imagery predominating in the waking state also predominates in the drowsy state. My purpose in the present experiment was to investigate this matter further by a new method and with a larger group of subjects than had hitherto been used, and to determine whether or not this transformation of type is a typical experience. That is to say, I wished to determine to what extent the *non-verbal* (2,9)[2] imagery present in the *waking* state is different from that present in the *drowsy* state. I was concerned primarily with the *presence* rather than with the *function* of imagery.

At the beginning of *The Experimental Psychology of the Thought-Processes*, after proposing to turn out his mind for inspection, Titchener states that he lectures from three main cues under different circumstances: he says he reads off what he has to say from a memory manuscript, follows the lead of his voice, or trusts to the guidance of kinaesthesis (13, 8). He claims that he may look to all three kinds of prompting in the course of a single hour, but that some one method is followed exclusively at times: "thus, when I am tired, I find that vision and audition are likely to lapse, and I am left alone with kinaesthesis" (13, 8 f.). Ordinarily, however, visual and auditory imagery are superior.

H. L. Hollingworth and L. S. Hollingworth made a study of their own imagery in drowsy states during the two years prior to 1911. Hollingworth found that "modes ordinarily vague and feeble become here dominant and vivid, even tending to replace customary imagery habits." He states further that "along with this emphasis of unusual modes goes the subordination of dominant modes, so that in the drowsy state, as in dream life, images even of these unusual types seem to exceed by far in intensity the clearest images of the waking state" (7, 104 f.).

Another very interesting observation has been made by Colvin. "I find not the least difficulty in recalling, mentally, rich colors, and I have often experienced, just before falling asleep, visual hallucinations of a most pronounced character. I have elaborate visual dreams and have been able to get a complementary image from a memory image. Yet I seldom employ imagery in my thinking. In learning a word series I seldom see it as printed

[1]This experiment was performed at Columbia University in 1920.

[2]The first number in parenthesis refers to the appended bibliography, and the other number, separated by a comma, refers to the page.

or written, my thinking being almost exclusively motor. Further, I have on several occasions experienced auditory hallucinations, but have, as far as I can discover, never had an auditory image. I recently experienced a most vivid taste dream, but I am reasonably certain that I have never used taste imagery in any of my thinking" (4, 235).

Martin gave imagery tests to three Stanford students who were in the habit of seeing apparitions (8). She claims the tests showed "most clearly that whether we will see ghosts at all and what their appearance will be depends upon whether we normally project our visual images into space or are able to do so under the influence of a strong emotional stimulus" (8, 251); but "the ability to project one's image is but one of the factors involved in the seeing of ghosts" (8, 257). Two of her subjects (Mr. M. and Miss T.) were predominantly visual in the hallucinatory state, and tests in the laboratory showed that they were also predominantly visual in the "normal" state. She does not give details for the third subject, but claims that he furnishes further evidence for the same conclusion.

The following statements by Binet (which are somewhat aside from the main thesis of the *Psychologie du Raisonnement*) have a bearing on our problem: "Ajoutons que très probablement les visuels sont spécialement prédisposés aux hallucinations de la vue, et conséquemment aux genres de délire dont les hallucinations de la vue sont le symptôme" (3, 20-21). Again: "Il est possible que les hallucinés de l'ouïe et les individus atteints du délire de persécution appartiennent au type auditif; et que la prédominance d'un ordre d'images crée une prédisposition à un ordre correspondant d'hallucinations—et peut-être aussi de délire" (3, 26, also 41, 118-125).[3]

These citations show that there is considerable disagreement on the subject.[4]

I do not attempt to deal with any "complex of images." On certain difficulties with the term 'mixed type' the reader is referred to Segal and Fernald (10, 124-132, 175-179; 6, 130-132).

The present investigation was begun with a group of 20 normal subjects, but I have included in this report only the results of the 8 subjects who became especially interested in the experiment and who served over an extended period. All 8 subjects had had some previous training in psychology, and most of them had had special training in introspection. Six of the subjects—A, B, C, D, E and F (Table I.)—were women, and two—G and H—were men.[5] I first gave each subject a series of "subjective" tests to determine to what extent his imagery was visual, auditory, kinaesthetic (cf. 11; 1, 77-79, 95-96), tactual, organic (cf. 1, 79-80, 97), olfactory, gustatory (cf. 1, 80 and 97), thermal and pain imagery.[6] I later checked up these results by certain objective tests (6; 1).

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[3] In the *Psychologie du Raisonnement* the term hallucination does not have exactly the same meaning as in more recent times.

[4] Cf. Peillaube's *Les Images*, especially 1-100.

[5] None of the subjects was spiritualistically inclined.

[6] My classification of imagery is obviously somewhat arbitrary, but it should be stated that it does not differ from other classifications in this respect. Some of the classes mentioned are rather unimportant introspectively, but each one of even the last 4 varieties named may occasionally predominate for a short time. I have not attempted to deal with verbal (or, some-

I attempted to evaluate the factors of frequency and vividness, in spontaneous as well as in voluntary imagery. Furthermore, I made an effort to determine the nature of the imagery present in the drowsy state. All 8 of the subjects, whose results I have included in this report, had drowsiness hallucinations rather frequently. Although first dependence was placed on the introspective method, I think the very general conclusions regarding the differences in imagery in the waking and drowsy states, which are stated at the end of this report, are fairly reliable.

II. Imagery in the Waking State

Any test whose purpose is to determine imagery types quantitatively is of course liable to various errors. Shall we judge a person's visual imagery to be superior to his auditory imagery, for example, because it is more vivid or intense, "more profuse and more readily summoned," or "more accurate and reliable" (6, 18)? In the absence of well-defined desiderata, the most wholesome attitude to take is to pay some attention, where practicable, to *all* of these qualities, realizing that any criteria are sure to be arbitrary to some extent. All of the various criteria which have been used from time to time rest on *a priori* reasoning, and the present study is no exception. We have relied for the most part on frequency and vividness (cf. 10, 214-218, 230-231). The judgment which I formed of each subject's imagery in the waking state is a composite of (*a*) his spontaneous imagery (see 6), (*b*) his reactions to a large number of "equivocal" stimuli which were designed to furnish a good opportunity for each kind of imagery being evoked at different times (cf. 10, 222-228), and (*c*) his ability to call up imagery of each variety under very favorable conditions (voluntary imagery: see 2). The results obtained by these three tests were then averaged, and the percentage-scores for the 5 most prominent classes of imagery are shown in the left-hand portion of Table I. Each of these three tests now deserves a separate description.

(*a*) *Spontaneous Imagery.* After each subject had been questioned and interviewed personally about his imagery, he

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times, articulatory) imagery, partly because of the possibility that this so-called verbal imagery is nothing but sensations derived from actual muscular movements. I readily grant the possibility that a similar explanation might be made for several other kinds of imagery which are to be found in my classification (cf. 5, 105-107). Wherever it can be demonstrated that this is the case, then to that extent the study had to do with resulting sensations rather than with images. It is quite well known that it is next to impossible to distinguish accurately between an image and a sensation on all occasions.

was supplied with a small blank-book in which he was to record his imagery from time to time by a variety of the "interrupted thinking" method. The subject "interrupted" himself in a variety of mental operations over a period of several weeks, and made a record of the kind and vividness of the images (if any) which were present. Vividness was rated on a scale of five.[7] Some of the tests devised by Betts (2, 54-87) and Fernald (6, 24-31) were used as ancillary aids in obtaining fairly representative mental operations. The final records obtained from several hundred interruptions take account of both frequency and vividness.

In this test, visual imagery was the most prominent class for all subjects except E, who had more kinaesthetic imagery. Visual, auditory and kinaesthetic imagery included 86 percent. of the total score made by all of the subjects. Visual, auditory, kinaesthetic, tactual and organic imagery included 94 percent. Olfactory, gustatory, thermal and pain imagery was relatively unimportant.

(*b*) *Test with Equivocal Stimuli.* The second test used in determining the imagery present in the waking state consisted of some 200 words and phrases, selected with the idea of giving the various kinds of imagery about the same opportunity of being evoked (10).[8] These 200 stimuli, which consisted of such expressions as "the motion of a boat," "laughing," "electric shock," "peeling an orange," "door knob," "fire, fire," "horse galloping," etc., called up different kinds of images with different subjects. They were exposed consecutively to each subject, who was instructed to record the *first* image (if any) which he experienced; but no image was recorded if it did not clearly appear at once. In order to determine the consistency of this test, it was given a second time to each subject one month later. There was a very close agreement between the scores made on the two trials one month apart. I have used the average of the two trials in the calculations.

In this test, visual imagery was easily the most prominent variety in the case of every subject. Visual, auditory and kinaesthetic imagery included 73 percent. of the total score made

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[7]The scale used in grading these images was as follows:

As clear and vivid as an actual sensation	5
Almost as clear and vivid as an actual sensation	4
Moderately clear and vivid as an actual sensation	3
Rather dim and vague	2
Extremely dim and vague	1

The subjects were instructed to pay particular attention to the 1st, 3rd, and 5th grades, using the 2nd and 4th only on occasion (cf. 1, 101-104).

[8]I am indebted to Professor R. S. Woodworth for this list.

by all of the subjects. Visual, auditory, kinaesthetic, tactual and organic imagery included 89 percent.

(*c*) *Voluntary Imagery.* The third test used in determining the imagery present in the waking state was a questionary, the purpose of which was to test the subjects' *ability* to call up each kind of image under very favorable circumstances. In this test I took advantage of the work of Betts, and followed his questionary (2, 20-24) in many details.[9] After various stimuli had been tried out on a group, I selected the 10 stimuli, for each of our 9 categories of imagery, which seemed to offer the best opportunity for evoking that particular kind of image. Clearness was again graded on a scale of five, the final score thus taking account of both frequency and vividness.

The most marked characteristic of the results obtained with this voluntary imagery test is the greater distribution of imagery.[10] The scores made in visual imagery range from 11 to 50 percent. for different subjects (median at 15.5); auditory imagery ranges from 4 to 18 percent. for different subjects (med. at 13); kinaesthetic from 2 to 17 percent. (med. at 10); tactual from 8 to 23 percent. (med. at 14); and organic from 6 to 18 percent. (med. at 13.5).[11] Visual and auditory imagery is much less prominent relatively than in the two other tests. Touch and organic imagery is relatively more prominent.

(*d*) *Average Score of the Three Tests.* The final judgment of the general nature of a subject's imagery was obtained by averaging his percentage scores in these three tests.[12] The average result is shown in the left-hand portion of Table I. The figures in the table give only a very general idea of each subject's imagery (cf. 6, 130), and are not to be considered as very accurate. It is fairly safe, however, to assume from these figures that subjects A, C, D and G are predominantly visual. In no case

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[9]The following directions will illustrate the type of questions used in Betts' questionary: "Think of seeing each of the following, considering carefully the picture . . . ; and classify the images suggested by each of the following questions as indicated by the degrees of clearness and vividness specified in the Key: . . . . The sun as it is sinking below the horizon . . . The stars on a clear winter night . . . " (2,22).

[10]This conclusion is in agreement with the work of Betts, who states that "most persons can command a far wider range and greater profusion of imagery than they normally employ in thinking" (2, 94).

[11]In his *Psychology* (4th ed.), 218, Angell writes that "the imagery which most of us find we can command with greatest accuracy and flexibility is that belonging to the perceptual process with which we are most intimately familiar, *i.e.*, vision, hearing, movement, and touch."

[12]We thus take account of the images which are *ordinarily* present, of the predisposition for certain kinds of imagery, and of the subject's *ability* to call up images of various kinds (cf. 10, 214).

does an average percentage score for a subject's olfactory, gustatory, thermal or pain imagery reach the figure 10, and such data are not given in the table.

If any changes should be made in weighting the three tests described above, I should be inclined to weight spontaneous imagery higher, because this type of test seems to fulfill the ideal conditions of a diagnostic imagery test better than any other. However, the general order of prominence would remain practically identical in every case. Weighting the percentage scores made in the spontaneous imagery test higher would simply *emphasize* the differences shown in Table I.

III. Imagery in the Drowsy State

We are dealing here with a group of phenomena which are allied with both the waking and dream states, belonging, as Ellis says, to the "ante-chamber of sleep." The subjects, whose records I include in this account, had drowsiness hallucinations rather commonly, and seemed able to distinguish fairly well between the drowsy and dream states. The plan which I adopted was to have each subject make a record of his drowsiness hallucinations over an extended period. The subject was supplied with a small blank-book and was given specific instructions in regard to the way these records were to be made. He was instructed to write out an accurate account of the hallucination immediately after he experienced it, regardless of whether it seemed individual, commonplace, or attractive,—and he was also cautioned not to *look for* any particular kind of imagery. He was especially cautioned to make a record of *all* of his hallucinations *immediately* after he experienced them, and to use much descriptive language. Judging from the assurances given me by all of the subjects, I am inclined to believe that the hallucinations reported are fairly representative.[13]

I later read all of the hallucinations recorded, and listed the kind, and sometimes the vividness, of the images which had probably been present. If a subject saw things in his drowsy states, this fact would find its way into his report. While language is sometimes a symbol for meaning (2, 8), such expres-

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[13]One advantage in this procedure is that no effort is made to direct the subject's thinking; and whatever may be lost by not being able to "set a definite problem" is more than compensated for by the factors of spontaneity and naturalness.

Since none of the subjects knew the purpose of the experiment, I think the factor of suggestion, commonly encountered in experiments of this nature, had practically an equal opportunity of operating for all kinds of imagery.

I was not interested in those images which were only implicitly present (cf. 2, 7-8).

sions as "I saw clearly," "I could barely hear," "I felt a whirling sensation," "My muscles strained as I jumped," and similar expressions, give some indication of the kind of imagery present. All of the subjects were interviewed by me every three or four days; and each subject continued to record his drowsiness hallucinations until I was reasonably certain that I knew the general nature of the imagery which he experienced in this state.

The right-hand portion of Table I. shows the distribution of imagery in the drowsy state. Visual imagery is in the lead for most subjects. The five categories of imagery shown in the table represent 97 percent. of the total score made by all of the subjects. Pain, thermal, gustatory and olfactory imagery is practically absent.

TABLE I.
Imagery in the Waking and Drowsy States

Subject	Percentage Score									
	Waking					Drowsy				
	V	A	K	T	O	V	A	K	T	O
A	67	8	2	9	3	47	16	14	12	4
B	39	24	15	5	6	50	39	7	2	1
C	57	10	5	11	3	93	7	0	0	0
D	33	15	16	9	11	30	37	11	5	12
E	30	9	27	9	12	19	42	23	1	14
F	31	23	13	10	9	47	29	18	6	0
G	36	11	12	5	14	35	15	23	17	4
H	31	10	13	11	5	44	50	6	0	0

The *letters* V, A, K, T and O in the above table refer to visual, auditory, kinaesthetic, tactual and organic imagery, respectively. The *figures* in the table represent percentage scores. For example, subject B has 39 percent. visual imagery in the waking state and 50 percent. in the drowsy state. The left-hand portion of the table gives the average score made in the three tests for imagery in the waking state. These three tests are described in Section II. In visual imagery, for example, subject B made percentage scores of 42 in spontaneous imagery, 58 in the test with equivocal stimuli, and 18 in voluntary imagery, giving a final average score of 39 in visual imagery in the waking state. The percentage scores for the 4 types of imagery not shown in the table (pain, thermal, gustatory and olfactory) constitute a very small percentage of the whole. The figures in the table possess only a very general value.

IV. Results

The data of Table I. are represented graphically in Figure I. The 5 varieties of imagery shown in the figure represent a higher percentage of the total score in the drowsy state than in the waking state. It is possible that this is a spurious result, however, because of the disturbing memory factor. In the case of several subjects, auditory imagery especially is more prominent in the drowsy state.[14]

[14]Ogden makes a similar observation in his *Introduction to General Psychology*, 81. It is interesting in this connection to observe that auditory hallucinations are prominent in several abnormal conditions (9, 30).

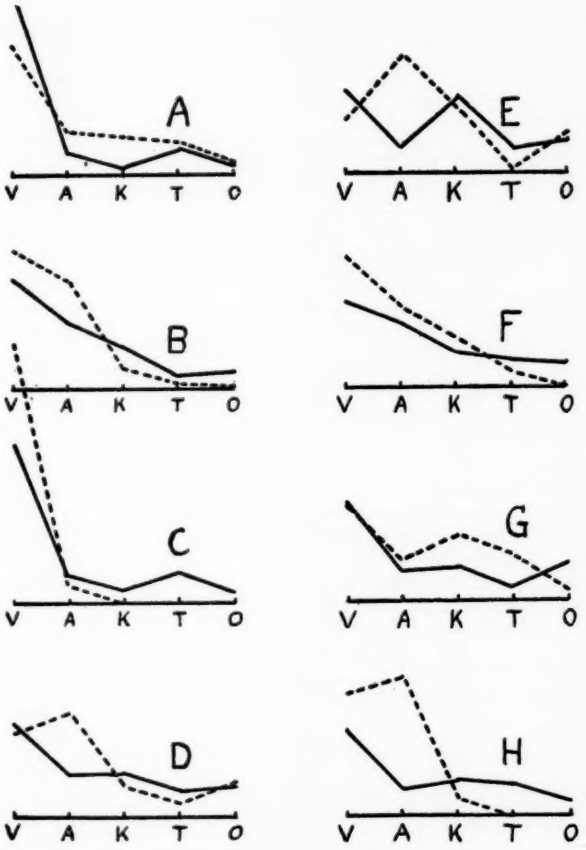

FIGURE I.
Imagery in the Waking and Drowsy States

The letters V, A, K, T and O in the accompanying figure, as in Table I., represent visual, auditory, kinaesthetic, tactual and organic imagery, respectively. The distances above these letters, on the base lines, show the percentage scores (1 mm. for every 2 per-cent.). The subjects are designated by the large capitals (A, B, etc.).

There is a well-marked shifting of certain types when passing from the waking into the drowsy state in the case of subjects E and H. Subjects B and F, on the other hand, maintain about the same order of prominence in the two states. We should naturally not be justified in concluding from these data either

that there *is* a common transformation of the dominant types or that there *is not*. The most marked characteristics which we observe are the individual differences between the various subjects and a general irregularity of the results (cf. 6, 20-21; 10, 218-222).

Most of the drowsiness hallucinations which the subjects reported were composed of the subject-matter of every-day life, with recency as the principal determining factor. A few of the hallucinations were grotesque in character, and some were like a "flash in the pan." There were no veridical hallucinations. The following may be given as a fairly common type of hallucination: "11:30 p. m. Just before falling asleep; in bed. Imagined myself stepping backwards off the subway platform. Reflex jerk of entire body. Image distinctly kinaesthetic, accompanied by visualization of the situation. I was not asleep, but nearly so."

The following hallucination illustrates the irrelevant variety: "12:30 a. m. Saw quite plainly a group of 3 men and 3 women standing around a mahogany table, discussing something. Then I heard one of the women say very distinctly, 'That being the case, I think I shall get drunk too.' The picture then faded completely." [15]

V. Imagery in Dreams

An effort was also made to study the dream state,[16] which is characterized by little power of co-ordination and often by a flight of images, but lack of time kept both subjects and experimenter from continuing the investigation. The method by which I attempted to determine the kind of imagery present in the dream state was the same as that described in Section III. The following data are admittedly not very reliable, because so few observations were made.

IMAGERY IN DREAMS[17]

Subject	Percentage Scores					Number of Observations
	V	A	K	T	O	
A	38	29	5	19	5	21
B	45	29	12	7	2	41
D	44	22	22	3	6	36
E	29	21	29	0	21	14
F	56	17	0	11	0	18

It might be pointed out that, with the exception of a few irregularities, there is a very general similarity between the nature of

[15] Many examples of hallucinations (of very unequal value) may be found in the lengthy English *Report on the Census of Hallucinations*, Proceedings of the Society for Psychical Research, 1894, 10, 25-422.

[16] A very interesting account of this state is given by B. Sidis: An Inquiry into the Nature of Hallucination, *Psychol. Rev.*, 11, 1904, 104-137. Some good examples of hallucinations are given by H. B. Alexander: Some Observations on Visual Imagery, *ibid.*, 11, 1904, especially 324, 328 and 329.

[17] The distribution of imagery shown in this table is in close agreement with that obtained by Thompson, who "dissected" 190 dreams of 5 sub-

the imagery present in the dream state and in the other two states, particularly in the case of subjects B and D.[18]

VI. Conclusion

1. The "transformation of imagery type," when passing from the waking into the drowsy state, is not a typical experience; and no general formula or rule can be stated which will hold for all cases. There are marked individual differences in this respect.

2. There is often a very noticeable increase in the relative prominence of auditory imagery when passing from the waking into the drowsy state.

3. There is a slightly greater similarity between the imagery present in the waking and dream states than between that present in the waking and drowsy states.

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jects (12). Thompson concludes that "that class of imagery is most frequent in dreams which is most frequently employed in waking life" (12, 305).

[18]Olfactory, gustatory and pain imagery was practically absent. Subject F had a score of 17 percent. in thermal imagery. So few observations were made by subjects C, G and H that I do not give their results.

THE APPROXIMATION OF ACTUAL DATA TO THE PHI-GAMMA HYPOTHESIS

By F. M. URBAN

L. B. Hoisington[1] published some time ago a short report on the results of a determination of the limen of dual impressions upon the skin. The data were fractionated in groups of 25, 50, 100, and 200 series. After calculating the constants h and c for every group, Hoisington determines the sums of the squares of the deviations of the actual percentages from the theoretical percentages. These numbers show the amount of agreement between the observed and the calculated results and are, therefore, a test of the degree in which the $\Phi(\gamma)$-hypothesis is realized in practice.

The results are given in two tables, one for each observer. The averages of the sums of the squares of the deviations show a highly uniform ratio of decrease. The mean variation of these sums also tends roughly to vary inversely with the number of series. These results seem to indicate that we approach the theoretical function as we increase the number of experiments. This is particularly true of Hoisington's observer D, and we must conclude that in his case the $\Phi(\gamma)$-hypothesis is approached with practice. The departure of the second 100 experiments from theory is only about one eighth of the first 100, and the last 50 fit theory over five times more closely than the first 50.

The $\Phi(\gamma)$-hypothesis can not fail to attract the interest of every worker in psychophysics, who will be grateful for any light thrown on this question. Hoisington's observation is all the more important, since it agrees with one made by Thomson[2]. In his research on the psychometric functions Thomson analyzed the results of my experiments on lifted weights and found a better agreement between theory and observation in the results of the more practised observers. Should further experience bear out this observation, we should be in the possession of a fact of far reaching practical and theoretical consequences.

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[1]L. B. Hoisington, An Example of the Fractionation of Data from the Method of Constant Stimuli for the Two-Point Limen, this JOURNAL, 28, 1917, 588–596.

[2]W. Brown and G. H. Thomson, *The Essentials of Mental Measurement*, 1921, 90 ff.

It is not likely that an investigation will be undertaken with the express purpose of testing Hoisington's observation. The amount of experimentation for settling this question would have to be very large, so large indeed that it might seem out of proportion to the point to be decided. At the present stage it is perhaps not even advisable to undertake such an investigation, since the possibility remains that Hoisington's and Thomson's observations are erroneous. A purely negative result is always disappointing, and it is particularly so if the investigation required a large outlay of time, money, and energy. At present it seems to be a case of incidental observations, and for this reason I wish to call the attention of investigators to this problem with a view to induce them to make similar observations.

I also wish to point out that a large material is at hand on which Hoisington's observation might be tested. Fernberger's doctorate thesis contains the data of two very extended series of experiments on lifted weights. The data are fractionated and the constants h and c are given partly in Fernberger's thesis, partly in my paper on the effect of practice in lifted weights. All that remains to be done is the calculation of the sums of the squares of the deviations, and this is a comparatively simple affair. A competent computer might do it in a day or two. My present circumstances prevent me from doing this work myself, but I hope that this suggestion may induce some person to undertake this small piece of research. The result is sure to be interesting. It is true that both of Fernberger's subjects were highly practised observers, in whom the effect of practice is less noticeable. One may expect to trace this influence, nevertheless, since the influence of practice on the value of the constants h and c is quite indisputable.

THE APPROXIMATION OF ACTUAL DATA TO THE PHI-GAMMA HYPOTHESIS

By Samuel W. Fernberger, University of Pennsylvania

In accordance with the suggestion made by Urban in the article just preceding, the calculations were made of the theoretical values of the curves of the 'greater' and 'less' judgments. The values h and c, the constants of the psychometric functions, are to be found in Tables 18 and 19 in Urban's article on practice.[1]

Table 1

Number	Lighter Judgments	Heavier Judgments
10	0.00528165	0.00569811
11	0.00206896	0.00579367
12	0.01018162	0.00275773
13	0.02570003	0.00439892
14	0.01102116	0.02515375
15	0.00309432	0.00806441
16	0.03949521	0.00661291
17	0.02073429	0.00225738
18	0.01522867	0.00881963
19	0.01508111	0.00991014
20	0.01071539	0.01572607
21	0.01239068	0.02139374
22	0.01329113	0.01770968
23	0.01522534	0.03618851
24	0.00434233	0.02198976
25	0.06417478	0.00495409
26	0.00189263	0.00458539
27	0.00026153	0.00115018
28	0.00888697	0.00055554
29	0.00143610	0.00500059
30	0.00364235	0.00033702
31	0.01978015	0.00582996
32	0.01312453	0.00199937
33	0.00771604	0.04510175
34	0.00660163	0.00859111
35	0.00862567	0.00356635
36	0.00960937	0.01359313
37	0.01315930	0.03513948

The observed relative frequencies are to be found in Tables 16 and 17 of the same article. The results of Urban's Observer VIII were calculated in full. This involved 28 groups of 50

[1] F. M. Urban, Der Einfluss der Uebung bei Gewichtsversuchen, *Arch. f. d. ges. Psychol.*, 29, 1913, 271–311.

judgments on each of 6 comparison pairs. Only the last 28 groups for Observer II were calculated. The first 9 groups, the constants of which are given in Table 18, were made by this observer in an earlier investigation, a number of years prior to his subsequent practice. The group numbers in our tables, given below, conform to the group numbers in Tables 18 and 19 of the Urban article.

TABLE 2

TABLE 19 NUMBER	LIGHTER JUDGMENTS	HEAVIER JUDGMENTS
1	0.02081909	0.00587067
2	0.00504132	0.01469981
3	0.00332283	0.00641695
4	0.03364268	0.01863544
5	0.01477663	0.00671550
6	0.00772576	0.00706596
7	0.01695835	0.01060964
8	0.00263027	0.00084990
9	0.01515844	0.00817609
10	0.03513519	0.04134780
11	0.00878956	0.00605952
12	0.00659333	0.00783328
13	0.01396908	0.01723052
14	0.00613113	0.00494092
15	0.00930283	0.00621563
16	0.00377251	0.00607283
17	0.00376331	0.00201803
18	0.00823965	0.00551722
19	0.01447011	0.00856419
20	0.00596861	0.01182627
21	0.00689667	0.00263735
22	0.02143435	0.00386525
23	0.01367870	0.00781873
24	0.01053391	0.00299834
25	0.00531802	0.01850315
26	0.00934552	0.04598085
27	0.00536269	0.00223346
28	0.00965603	0.00407375

We had the theoretical curves calculated for the greater and less judgments for the stimulus values corresponding to those used in the investigation. These calculated values were then compared with the observed relative frequencies of judgment, and then the sums of the squares of the deviations were calculated. These sums of the squares of the deviations are given in Tables 1 and 2. The results of Observer II are given in Table 1, and these correspond to the values found in Urban's Tables 16 and 18. Table 2 contains the values for Observer VIII and corresponds to Urban's Tables 17 and 19.

The results seem to verify the observations of Hoisington[2] and Thomson[3] in that there is a closer approximation of the observed to the theoretical curves of the psychometric functions as a result of practice. The difference seems to be more marked for Observer VIII than for Observer II. This result might have been anticipated because Observer II was relatively more highly practised at the beginning of experimentation, having participated as observer in a former investigation. This also is evident from a study of the magnitudes of the constants of the psychometric functions which we have made previously for these same two observers.[4]

The decrease in the size of the sums of the squares of the deviations is far from regular but the general tendency seems evident. This tendency is more marked than the values indicate, as an analysis shows that a large sum of the squares in the earlier series is invariably due to large deviations on at least three or four of the six observed frequencies. Large deviations in the later series, such as that for Group 26, Heavier Judgments, Observer VIII (Table 2), are almost invariably due to a large deviation (frequently an inversion) for one of the observed frequencies only.

In the results of Thomson and especially in those of Hoisington the decrease in the size of the sum of the deviations is very much more marked than in the present results. This is because Hoisington used entirely untrained subjects and calculated from the first judgments obtained. For the present values, the observers had reached a fairly high degree of practice before the judgments were recorded.

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[2]L. B. Hoisington, An Example of the Fractionation of Data from the Method of Constant Stimuli for the Two-Point Limen, this JOURNAL, 28, 1917, 588–596.

[3]W. Brown and G. H. Thomson, *The Essentials of Mental Measurement*, 1921, 90ff.

[4]S. W. Fernberger, On the Relation of the Methods of Just Perceptible Differences and Constant Stimuli, *Psychol. Mon.*, No. 61, 1913, 41–45.

ALCOHOL, TOBACCO AND TEA: THE EFFICIENCY UNDER DRUGS AS DERIVED FROM THE LEARNING EQUATION

By Max F. Meyer

Let no one get from the title the erroneous impression that this report is concerned with the problem whether a person's learning ability may be changed by drugs. That is not the problem here. The *learning* equation enters into the problem only incidentally, but *unavoidably, as we shall see.* The purpose of the experiments was to determine *the relative efficiency in doing work* under the influence of various drugs.

Not a few people can be found who have the habit of taking, about half an hour before going to work, a particular drink. Some of them expect no special consequence therefrom. Some, on the other hand, take that drink intentionally as a "tonic." That is, they expect that that drink will either increase the efficiency of their work or at least make them more happy and contented during the performance of the task.

Others have the habit, not of taking a particular drink as a tonic for their work, but of smoking tobacco before or rather during their work.

Still others have, not the habit, but what they would call the unfortunate fate, of being compelled now and then or frequently to work in rooms where the air is filled with tobacco-smoke. If they are not smokers themselves, they feel then very uncomfortable during their work and naturally expect from such a condition, in addition to other consequences which do not concern us here, a diminished work-efficiency.

The present study was undertaken in order to throw more light on the questions which are suggested by the contents of the preceding paragraphs. No attempt was made to solve or even only to formulate any specific problem in neurology or pharmacology. The problem and the procedure adopted to solve it resembled a problem of ordinary business. But the procedure had nevertheless its scientific aspects. First, a kind of work was chosen which permitted a large number of repetitions under virtually identical conditions. The repetitions permitted the elimination of the influence of the (known as well as unknown) chance factors. Secondly, the conditions under which the work was done were held as identical as possible except where they were intentionally varied. Thirdly, a record was regularly taken of the time required for the work. Fourthly, the procedure and the computations made were based on a

previous analysis of the learning equation as discussed in another paper.[1]

The "business" whose efficiency was to be observed consisted in writing shorthand exercises. The "worker" did not write these exercises in order to please somebody interested in some scientific problem. Neither did he write them because he was paid for the work by the hour. He wrote them in order to do himself a favor. He did not have to whip himself by suggestion, loyalty, gratefulness or other artificial means into an artificial state of attention and interest. He was actuated by the most reliable motive, sheer selfishness. He *wanted* to do that very work for his own well-understood benefit, and to do it with the least loss of time.

The first worker, whose records were taken in the summer of 1921, was a middle-aged man, who had used weak alcoholic drinks now and then in his youth, but so rarely that even in that period of his life he could be said to have been an abstainer. He had been a total abstainer during the last twenty years. He had never smoked in his life. He did not customarily drink either tea or coffee or "cokes" and similar drinks.

He had learned a certain system of shorthand, a little over a year before, to the extent of knowing all the rules of the system, and of being able, therefore, to read and write anything in that system. But he could do that generally only very slowly, and he was anxious to improve his reading and writing speed by systematic exercise. He procured a list of either most frequently occurring or (with respect to the rules) most typical words, well illustrating that whole system of shorthand. This list he copied just once at each sitting. To be more exact: he looked at each word (or later, with increasing practice, whenever possible, small groups of words) as it appeared in shorthand in that list until he had "read" it, then turned his eyes to his copy book and, from memory or aided by his knowledge of the system of rules, "wrote" it *with amply sufficient neatness* to feel sure that he could again easily read it if that might be necessary at any future time.

The unit of time in which (in this first experiment only) the duration of each work-period was recorded, was the rather large unit of .5 min. It was thought that, in work of this kind, where a pencil may have to be exchanged, a page may have to be turned, a readjustment of the sitting posture may have to be made, that is, in work which is so much like ordinary business, a very small time unit would not commend itself to reason. The measuring stick need not be finer than the tools with which the work is done. However, experience showed that a smaller unit, say, .1 min., would have been better; and seconds might then as well

[1]This JOURNAL, April, 1923.

have been read off the watch, even though the accuracy of the experiment does not go as far as that. (Seconds were used in the later experiments.)

The worker had the habit of eating only two meals a day, breakfast and supper. Being thus without occupation during the lunch period, he chose that time of the day for his practice period. Since everybody else who might otherwise have disturbed him was at that time engaged elsewhere, he was able to do his work without any interruptions.

From 20 to 30 min. before starting his work he took his "tonic." He used four different kinds, sugar, tobacco, alcohol, tea. He used them on successive days in the order named; on the fifth day he started again on sugar, and so forth. He tried to work regularly on all the successive days of the week; but now and then (not often) he had to drop a day on account of duties taking him elsewhere. The work seemed to progress on the days following such an intermission so nearly in the same way as if that intermission had not occurred, that no attention is paid in the following discussion to these intermissions.

If the intermission could have had an unfavorable influence on the efficiency of the work of the day immediately succeeding, it would have been unfavorable about equally to the showing made by the different "tonics." Maybe the sugar, and even more the tea, should have made a showing a trifle better than our following records.

In the numerous experiments after this first—let it be said at once—there were virtually no intermissions, and this matter will therefore not be referred to again.

The tea was prepared by placing 5 gr. of green tea leaves in about a pint of boiling-hot water together with 20 gr. of sugar, and removing the tea-leaves after 6 min. by filtering.

The alcohol was prepared for drinking by mixing 10 cu. cm. with about a pint of cold water together with 20 gr. of sugar.

The tobacco was administered by placing 30 gr. 30 min. before the beginning of the work on a hot iron plate, where it turned to ashes in about 20 min., filling the room with smoke. The worker, of course, remained in the room from the moment he put the tobacco on the plate, and did not enter the room only for the beginning of his work 30 min. later. The size of the room was 22 by 16 ft. and the height 18 ft. The windows were closed in order to confine the fumes, but the door was kept open all the time so that there was at least that amount of ventilation which one is apt to find in rooms where men smoke and work at the same time. The burning tobacco was about 12 ft. from the chair on which the worker sat occupying himself with reading and the like while waiting for the proper time for the beginning of the "business." The room was still very smoky after the business was done, an hour later.

The sugar was taken 20 gr. in a cup or two of water. Of course, no particular effect was expected from the sugar; but since the worker "took something" always 30 min. before starting his work, it was thought that sugar would be about the most innocent thing that he could take and still call it "something."

The worker did not like (or rather, greatly disliked) either the tea-drink or the alcohol-drink or the tobacco-fumes. He called all of them "vile stuff." He felt uncomfortable all the time while being in the tobacco fumes. And under the influence of any of these three substances he complained of being slightly nauseated for an hour or two.

The selection of the particular kind of work, a shorthand exercise, was not purely the result of the fact that this worker regarded this as very useful for him and was therefore very eager to do it,—a fact which does not hold true for all psychological tests described in the literature. The shorthand work in this case was truly useful work. But, in addition, any shorthand work done for practice is truly intellectual work. It is not so exclusively memory work as, for example, adding or subtracting or multiplying numbers.

The shorthand work requires not only a knowledge of a number of rules (to be applied mechanically like the multiplication table), but also a considerable amount of "choice" in the application of each rule whereby to obtain in each individual case (of a word or group of words) a greater or lesser amount of economy, whichever the writer may be satisfied with. These intellectual processes play their rôle both in reading and in writing shorthand.

May it be repeated here what has been said on another occasion?[2] One serious deficiency of the work done hitherto (usually under the pressure of somebody wanting immediate results) on efficiency under drugs consists in largely or totally avoiding those processes which are "subject to improvement during continued testing. They are the very processes that interest us most. An artist drinking a bottle of wine before going to work does not intend thereby to improve his knee-jerk. And the student who drinks a cup of tea before a mathematics examination does not intend thereby to improve his use of the multiplication table. The highest intellectual processes ought to receive the chief attention of the experimenters of the future."

Since the improvement popularly called "learning" plays its *foremost* rôle in the highest, the *intellectual* processes of human life, it is *prerequisite* to develop a method of describing in accurate (mathematical) terminology the *efficiency in successive stages of the learning process under normal or average conditions,*

[2]Max F. Meyer, The Psychological Effects of Drugs, *Psych. Bulletin*, 19, 1922, 173–182, esp. 178.

before we can make a comparison of the efficiency under drugs. This prerequisite the writer has fulfilled in another place, in his article on the learning equation, to which reference has already been made and will further be made.

The procedure in this experiment has for the most part already been explained. A few details still have to be added to what has been said. At first, 30 min. were consumed in writing the list of selected shorthand words. But after a week that time had been reduced to nearly one-half of the original time. A second practice with different material was therefore taken up immediately on completing the first. The first is in the following tables referred to by A and the second by B.

The second practice, B, consisted in copying, not a logically disconnected list of words as in the first, but a few commercial letters found in the shorthand text-book. After a few more days a third practice, and again after a few days a fourth practice, were added. These are in the following called C and D. They also consisted in typical, brief commercial letters.

Toward the end of the whole experiment, lasting about 6 weeks, each group of the 4 practice periods A, B, C and D combined with short intervals for the necessary adjustments filled a little over an hour of the worker's time.

The constant L, defined as the individual learning ability, has been taken in all the computations concerning work of this same individual as 20.1. No attempt at any extraordinary accuracy has been made in determining the other three constants, T, p and a. None of these constants is wanted for its own sake in the experiment under discussion. They are wanted only in order to make possible the substitution of a computable curve for the experimental curve.

TABLE A

L = 20.1, T = 21.0, p = 2.4, a = 1.3.

Sugar			Tobacco			Alcohol			Tea		
n	t c'p't'd	t exp'l	n	t c'p't'd	t exp'l	n	t c'p't'd	t exp'l	n	t c'p't'd	t exp'l
0	60.7	60	1	49.4	52	2	43.1	46	3	39.1	36
4	36.3	34	5	34.2	30	6	32.7	32	7	31.4	30
8	30.4	32	9	29.6	30	10	28.9	32	11	28.3	29
12	27.8	34	13	27.4	28	14	27.0	34	15	26.6	26
16	26.3	28	17	26.1	27	18	25.8	27	19	25.6	27
20	25.4	29	21	25.2	27	22	25.0	27	23	24.9	24
24	24.7	28	25	24.6	25	26	24.5	26	27	24.3	23
28	24.2	25	29	24.1	22	30	24.0	24	31	23.9	23
32	23.9	27	33	23.8	24	34	23.7	25	35	23.6	23
36	23.6	23	37	23.5	24	38	23.4	25	39	23.4	20
40	23.3	24	41	23.3	23	42	23.2	24	—	—	—

The above Table is immediately clear to anyone familiar with my previous article on the learning equation, in which the definitions of all the terms are given. With the actual computation I hesitate to bore the present reader. If he wishes to "audit" my computation he may, if he is a teacher of psychology, give the computation to some of his students. It is good practice for somewhat advanced students. Good practice for them, too, it will be to plot both the computed curve and the experimental curve and to discuss whether one should vary slightly p and a from the values given above in order to obtain a closer approximation of the computed curve

to the experimental curve. As was said before, however, for our present purposes no very close approximation is needed.

Let it be pointed out to those who wish to make the computation, that they must first find a suitable value of K. We proceed as follows. First we plot the experimental curve and make a guess with respect to its probable fall until n becomes very large. It looks like a fair guess that 21 would be about the asymptotic limit; that is, that T is about 21. Since K is equal to the product of (L–1) and T, divided by 180°, and since we found for this individual in our previous article L equal to 20.1, K must equal about 19.1 times 21 divided by 180, or 401.1 over 180, or about 2.23. Let us accept this provisionally. Table A proves that T equal to 21 was indeed a fair enough guess.

In order to utilise the four columns of the Table for comparative purposes, we *compute now each experimental value as a certain per cent. of its computed value.* That is, we divide each experimental t by the corresponding computed t and multiply the quotient by 100. For example, we divide 60 by 60.7 and get 99%, we divide 52 by 49.4 and get 105%, we divide 46 by 43.1 and get 107%, and so forth. I do not wish to bore the present reader with the enumeration of all the results per cent. I give below at once the average for each of the 4 columns of the table, that is, for each of the 4 drug conditions.

Summary of Experiment A

Alcohol	107	(98 to 126)
Sugar	106	(94 to 122)
Tobacco	100	(88 to 107)
Tea	96	(86 to 106)

I have also given the extremes (in parenthesis) for each of the four drug conditions. *No meaning at all is conveyed by the averages (or the extremes, either).* They are not intended to convey any meaning by themselves. But compared with each other they have some significance. The values 107 and 96, for instance, can be said to mean this: on an average "tea day" the shorthand writer worked about 11% more efficiently than on an average "alcohol day." The "tobacco days" have an intermediate position. Little difference is apparent between the "alcohol days" and the "sugar days." Nevertheless, the alcohol may have placed the worker at a slight disadvantage in comparison with the sugar.

TABLE B

L = 20.1, T = 26.0, p = 2.8, a = 1.2.

Sugar			Tobacco			Alcohol			Tea		
n	t	t	n	t	t	n	t	t	n	t	t
0	71.8	68	1	60.1	60	2	53.2	54	3	48.5	48
4	45.3	52	5	42.8	46	6	40.9	40	7	39.4	42
8	38.2	43	9	37.2	35	10	36.3	41	11	35.5	34
12	34.9	43	13	34.3	32	14	33.8	35	15	33.4	31
16	33.0	34	17	32.7	31	18	32.3	33	19	32.0	31
20	31.8	32	21	31.5	29	22	31.3	33	23	31.1	32
24	30.9	34	25	30.7	29	26	30.6	32	27	30.4	28
28	30.3	29	29	30.1	28	30	30.0	31	31	29.9	27
32	29.8	29	33	29.7	28	34	29.6	30	—	—	—

Summary of Experiment B

Sugar	106	(95 to 123)
Alcohol	104	(98 to 113)
Tea	97	(90 to 107)
Tobacco	96	(92 to 107)

In order to have a suitable K for the above computation, we first had to make a guess as to what T might equal. We guessed at T equal to 26. K is then 19.1 times 26 divided by 180, or 496.6 over 180, or 2.76. The actual computation of the Table proves that the guess was fairly good,—good enough for our present purpose. In order to utilize the four columns of the Table for comparative purposes, each experimental t has again been divided by the corresponding computed *t*. The Summary gives us again the average of these values per cent. for each column of the Table, and, although these averages are quite meaningless for themselves, as already said, they permit us to measure the relative efficiency.

Here, in Experiment B, the sugar days make a showing slightly poorer than that of the alcohol days. And tea and tobacco also have exchanged places. But there is no doubt that tea and tobacco belong to the same class as compared with sugar and alcohol, which belong to a different class, the former two representing greater, the latter two lesser efficiency.

TABLE C

L = 20.1, T = 34.0, p = 2.1, a = 1.8.

Sugar			Tobacco			Alcohol			Tea		
n	*t*	t	*n*	*t*	t	*n*	*t*	t	*n*	*t*	t
0	87.4	86	1	70.6	73	2	61.9	59	3	56.5	54
4	52.8	55	5	50.1	49	6	48.2	54	7	46.6	43
8	45.4	47	9	44.4	44	10	43.5	44	11	42.8	43
12	42.2	46	13	41.6	43	14	41.1	44	15	40.7	39
16	40.4	37	17	40.0	40	18	39.7	39	19	39.5	36
20	39.2	40	21	39.0	35	22	38.8	39	23	38.6	35
24	38.4	37	25	38.2	36	26	38.1	39	27	37.9	37
28	37.8	38	29	37.7	33	30	37.6	38	—	—	—

Summary of Experiment C

Alcohol	102	(95 to 112)
Sugar	101	(92 to 109)
Tobacco	97	(88 to 103)
Tea	95	(91 to 100)

Having guessed T in Experiment C to be about equal to 34, we find K to be 649.4 over 180, that is, we find K to be equal to 3.61. The Table proves that our guess at T was good enough for our purpose. The summary of Experiment C gives us the average of the ratios per cent. for each of the 4 columns of Table C. The 4 drug conditions have again the same relative positions as we found them in Experiment A. The advantage of tea over alcohol, however, here amounts only to 7%.

TABLE D

L = 20.1, T = 27.0, p = 3.6, a = 1.3.

Sugar			Tobacco			Alcohol			Tea		
n	t	t	n	t	t	n	t	t	n	t	t
0	61.6	60	1	54.2	52	2	49.4	54	3	46.1	46
4	43.6	43	5	41.7	40	6	40.1	41	7	38.9	37
8	37.9	42	9	37.0	39	10	36.3	38	11	35.7	33
12	35.1	34	13	34.6	32	14	34.2	35	15	33.8	36
16	33.5	34	17	33.2	33	18	32.9	33	19	32.6	31
20	32.4	31	21	32.2	33	22	31.9	33	23	31.8	30
24	31.6	32	25	31.4	29	26	31.3	31	—	—	—

Summary of Experiment D

Alcohol	103	(99 to 109)
Sugar	100	(96 to 111)
Tobacco	97	(92 to 105)
Tea	97	(92 to 107)

In D the value of T was provisionally taken as 27. And K was therefore 515.7 over 180, that is, 2.87. We see in the summary of Experiment D that the 4 drug conditions have again the same relative positions as we found them in Experiments A and C, except that tobacco and tea are tied. It is also worth calling attention to, that Experiment D consists only of 27 measurements, from 6 to 7 for each drug, and that the small efficiency-difference of only 6% between alcohol and tea may partly be due to the drug-effect being already on the decline, since about 90 min. had passed from the moment of taking the drug to doing the work.

Summing up the results of all four experiments, A, B, C, D, we can very well draw the conclusion that tea and tobacco belong to one class and sugar and alcohol to another class (or, maybe, the latter two to two other classes); that even greater efficiency results from tea than from tobacco; and that there is a certain suspicion that even this small dose of alcohol diminishes the work-efficiency in comparison with sugar.

Of course, the alcohol dose in these experiments must be pronounced exceedingly small. This suggested the taking of a stronger dose in a future experiment.

The worker, when asked if he was not anxious now, after seeing the results, to take to tea or tobacco or both, replied in the negative. One must remember that the general hygienic, not to mention the social, problem of the use of drugs is in no way touched upon in the performance of these experiments. It is highly probable that the increased efficiency is somehow paid for (not in money,—that is not the question) physiologically. And it is not unlikely that the bargain is not worth the price paid.

A very remarkable fact is this, that the worker, during and after virtually every tobacco or tea work, said to himself something like this: I certainly am working slowly; a mysterious

weight seems to force me to drag it along, and thus seems to keep me from going forward as fast as I might otherwise. And then, when he looked at the outcome, he found to his surprise that he had worked much better than he expected. On the other hand, the work of the alcohol days seemed to fly ahead and pull the operator after it. And then, when he looked at the outcome, he found to his surprise that his efficiency was much poorer than he had expected. It seems clear that a worker who lacks means of objectively measuring his efficiency can not trust his own feelings in the matter.

Let it be recalled in this connection that all these drugs except the sugar were nauseating to the worker, not only at the moment of taking them, but until the end of the time spent in the work. Especially the tobacco fumes were very disagreeable to him. But no such experiences, obviously, can be regarded as indicators of a worker's efficiency. This is entirely in accord with Tinker's remark[3] that "intended distractors may fail to distract" and with my own teaching[4] as expressed, for example, in the following quotation: "He who thinks for the first time of the deflection, in the nervous system, of a current by another current, may be struck by the effect which it has on the deflecting current. Is not the deflecting current strengthened thereby? . . . A little noise and a little glitter on the side do not distract, but help." Let us remember that the worker eagerly wanted to do those shorthand exercises and had made up his mind to do them, "rain or shine." It may even be that the "helpful" effect of the "distraction" accounts (although the present writer does not hold this view very strongly) for the unexpectedly high efficiency (*i.e.*, unexpectedly small inefficiency) of the alcohol days.

Some remarks about the behavior of the constant "a," the practice value of the task, and incidentally about the other constants, may not be entirely out of place here, although they have no relation to the drug effects, but only to the method of interpreting the drug effects on the basis of the learning equation. It is most plausible to assume that a drug affects of all the variables and constants of the learning equation only the variable *t* and the constant T.

There is no reason for assuming that a drug could affect the constant L, the individual learning ability, which is (or, which I regard as) an inherited physiological (functional) property of the "conductive" (nervous) tissue of that particular individual, called by me the "susceptibility" of his nervous tissue,[5] especially of his so-called higher centers.

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[3] This JOURNAL, October 1922, 580. See there other bibliographical references.

[4] *Psychology of the Other-One*, 98–116.

[5] *Op. cit.*, 130. Do not confuse "susceptibility" with "conductivity"! They are constants entering into the individual endowment which are entirely independent of each other. Either of these constants may be low while the other is high.

No influence of the drug is imaginable on the constant p, since this refers only to the individual's past, both his ancestral past (heredity) and his individual past (experience). No influence of the drug is imaginable, either, on the constant a, the practice value of the task.

It has already been mentioned that no attempt has been made to determine the constants p and a with any extraordinary accuracy relative to each other. The reason appears in the preceding paragraph and may also be stated thus: actual computation shows that, if "p" is taken a little smaller, this can *virtually* be compensated for by taking "a" a little larger, provided we remain within a limited range of n, as we naturally do, since we should not desire to repeat the same task thousands of times. By "virtually" we mean that the shape of the curve *within these limits of n* would vary very slightly,—not enough to trouble our interpretation of the drug effects. While therefore a was not determined with any great accuracy, the 4 values of a in the experiments A, B, C and D nevertheless agree fairly with a certain conclusion that seems possible *a priori*. By this statement is meant that this conclusion is possible *from the definitions* of the constants a and T. Is it not plausible that, when the practice tasks are made up of quantities of rather homogeneous unit materials, such as average words or syllables, the practice values are simply proportional to these quantities? If that is so, one may also say that the T's should turn out proportional to those quantities and then also proportional to the a's.

This indeed is nearly so, in the experiments B, C and D. The exercises here were ordinary commercial letters. The longer the letters, the longer T, and the greater then the value of a. The T's appear as 26 in B, 27 in D and 34 in C; and the a's appear as 1.2 in B, 1.3 in D and 1.8 in C. That looks like proportionality, considering that the a's were not determined very accurately and that the material was not entirely homogeneous. The largest a belongs to the largest T and the smallest a to the smallest T.

On the contrary, no such proportionality is found in experiment A with its T equal to only 21 and its a of the relatively high value 1.3. The material used in A was not homogeneous with the material used in B, C and D. It did not consist of ordinary sentences with their random aggregations of words, but exclusively of words for which the inventor of the shorthand system had devised special methods of abbreviation. It appears that this fact increased the practice value of the task.

The significance of these remarks about the constant a consists in being an answer to the question whether the definition which I adopted for a in the learning equation was a proper definition. The choice of an equation and the definition of its variables and constants is never a matter arbitrary, speculative and metaphysical, but one that has to justify itself by being directly or indirectly serviceable for practical purposes.

This, however, is not the place to enter further into a discussion of the learning equation as such and of the problems of learning. Let us be satisfied here with the observation that the definition adopted for the parameter a has thus far justified itself.

The results of further experiments on working efficiency under drugs will be published in a following article.

THE PSYCHOANALYSTS' THEORY OF THE CONFLICT-NEUROSIS

By L. L. Bernard, University of Minnesota

Of the many theories developed by the psycho-analysts, as an outgrowth of the psychotherapeutic methods of Freud, that of the conflict-neurosis is probably the best known and the most valuable to science. Having conflicts and complexes about things has come to be almost a fad among those who are in want of something exciting. But the conflict is also a serious fact of psychopathology, and has to be dealt with seriously by the worker in medical psychology and sociology. Neuroses and psychoses are particularly likely to develop in an over-complicated and hyper-stimulated society, where regimentation is in excess of rational social organization. Sociology and the social sciences generally have not yet discovered the method of smoothly taking care of the complicated adjustments in our modern world. Possibly, as some of the psychoanalysts believe, we shall never be able to learn this art. They hold that the breach between acquired culture and instinctive human nature has become so great that it never can be bridged. To a certain class of psychosocial theorists civilization has come to be synonymous with repression, or a disease. They believe that we are already too highly civilized, and would have us return to nature —or to the guidance of instincts—instead of moving farther in the direction of an artificially constructed and controlled civilization.

In the past century and a half—especially since the coming of the industrial revolution—there has been an abundance of social theorists to urge a cure for civilization. Rousseau, Fourier, Bakunin, and their kind, merely voiced a social philosophy for which Freud and his disciples have offered a psychological justification. I believe that the fundamental character of this conflict between instinct and culture has been much exaggerated. I have attempted to show elsewhere that the psychologists have overstressed the importance of instinct as a factor of control in our lives.[1] In this and in two other articles on the concepts of psychoanalysis[2] I have sought to offer some proof that our conflicts are not alone between instincts and culture, but are also in large part—perhaps primarily—between contending

[1]The Misuse of Instinct in the Social Sciences, *Psych. Rev.*, March, 1921.

[2]Instinct and the Psychoanalysts, *Journal of Abnormal Psychology and Social Psychology*, Jan.-Mar., 1923; A Criticism of the Psychoanalysts' Theory of the Libido, *Monist*, April, 1923.

cultures or acquired attitudes and interests. Perhaps, after all, we have more need to worry about the control of habit-formation under the influence of environmental pressures or education in the large than about instinct. The neuroses, coming as a result of conflict, are realities, and they must be dealt with; but possibly they are in the last analysis as much or more a problem for the sociologist than for the psychologist and the geneticist. It may be possible to remove a very large part of the conflict-neuroses by controlling the environmental pressures which build our human natures, and instinct may prove to be a flexible and malleable instrument in the service of civilization instead of an obstruction in its way.

The main contention of the conflict-neurosis theory is, to put it briefly in its most orthodox form, that "the neurotic symptoms are substitutions for sexual satisfaction."[3] Freud further states his theory of the causation of the neuroses as follows: "First, the most generalized factor, self-denial, then the fixation of the libido, by which it is forced into certain directions, and thirdly, the tendency to conflict in the development of the ego, which has rejected libidinous impulses of this kind."[4] It is at this point, according to Freud, that substitute activities or emotions are taken on, in compensation for the denied sex satisfactions. Sometimes these substitutions are in the nature of sublimations; but "the measure of unsatisfied libido which the average human being can stand is limited. The plasticity and freedom of movement of libido is by no means retained to the same extent by all individuals; sublimation can, moreover, never account for more than a certain small fraction of the libido; and finally, most people possess the capacity for sublimation only to a very slight degree."[5] Thus, where people are not able to find normal substitute satisfactions or to sublimate the "libido" they "become neurotically ill . . . ill with 'denial' . . . and . . their symptoms are the substitutes for the denied gratification."[6] These pathological substitutions which constitute the neuroses are too numerous to be detailed in a brief article.

It will be observed from the foregoing that Freud identifies the "libido" with sexual impulse.[7] He does not, however, deny that theoretically other impulses may play a part in the causation of psychoneuroses, but he maintains that he has not found any such cases:[8] "the essence of these maladies lies in disturbances of the sexual processes, in those processes in the organism which determine the formation and utilization of the sexual libido."[9] The conflict which leads to the neurotic satisfaction is never between sex-impulses or between ego-impulses as such. "The pathological struggle is waged between ego-impulses and sexual impulses."[10] This passage must be interpreted to mean that the conflict-neurosis arises out of the struggle of the sex-impulses for satisfaction against all other controls, which are here termed the ego-impulses; because Freud, having been educated in the strict hedonistic metaphysics and the subjective psychology of the Viennese school, does not recognize social or herd-impulses. He furthermore makes

[3] S. Freud, *General Introduction to Psychoanalysis*, 266. See also 260, 311, 373.

[4] *Op. cit.*, 305. [5] *Ibid.*, 300. [6] *Ibid.*, 299.

[7] See also *op. cit.*, 357–8.

[8] Freud, *Selected Papers on Hysteria and Other Psychoneuroses* (tr. A. A. Brill), 126, 141.

[9] *Ibid.*, 192 f.

[10] Freud, *General Introduction to Psychoanalysis*, 303. See also 304, 358.

an artificial distinction between the sex-impulses and the ego-impulses on the ground that the former often seek pleasure at the expense of the organism's welfare and safety.[11] The psychologist with a social point of view would tend rather to group the sex-impulses as a subdivision of the ego-impulses, and to set both over against a class of social or ethical impulses. These last, of course, would be considered mainly as acquired, at least in their functioning forms; but apparently Freud does not hold that even the sex-impulses are wholly inherited.[12]

Freud has an interesting justification of his theory of the limitation of the conflict-neurosis to the struggle of the sex-impulses for satisfaction against the censorship of the ego-organization. "It is worth noting that sexual instincts and instincts of self-preservation do not behave similarly when they are confronted with the necessities of actuality. It is easier to educate the instincts of self-preservation and everything that is connected with them; they speedily learn to adapt themselves to necessity and to arrange their development in accordance with the mandates of fact. That is easy to understand, for they cannot procure the objects they require in any other way; without these objects the individual must perish. The sex-instincts are more difficult to educate, because at the outset they do not suffer from the need of an object. As they are related almost parasitically to other functions of the body and gratify themselves auto-erotically by way of their own body, they are at first withdrawn from the educational influence of real necessity."[13] It is apparently true that the conflict of sex-impulses with other interests does cause a very large proportion of the psychoneuroses, but this result is scarcely due to the cause which Freud assigns. The main reason is that the sex-impulses are more firmly rooted in actual organic instincts, while the ego or self-preservative tendencies are in much larger proportion acquired. The objective unity of action here is imputed rather than real, and the subjective unity of concept is abstract rather than concrete and motor.[14] Consequently there is not such a strong tendency for the ideas connected with most of the other ego-impulses and desires to go over into action, and, being in the main acquired, they have more associations with other action-patterns into which they may be transferred. Doubtless, also, the fact that sex-desire and impulse can end subjectively much more easily than the ego-tendencies which are dependent upon (and usually derived from) the organization of the external psychosocial environment, which has in large measure given them form and content, has considerable influence here. The greater educability of the so-called ego-tendencies is due to the fact that they are primarily, at least in their "higher" and less vegetative forms, the product of environmental pressures and adaptation or education. Thus the distinction which Freud draws between these two sets of tendencies or impulses is seen to rest on the difference in degree of innateness which they possess.

Freud's is the orthodox view of the conflict-neurosis, and is concurred in by a very large portion of the psychoanalysts.[15] White's theory of the conflict-neurosis is somewhat different. For him the conflict arises between two "libidos," instead of between the libido (sex-impulse or desire) and the self-preservation values (interest), as maintained by Freud. It would be difficult to imagine a more metaphysical and obscurantist statement of the theory of conflict than that made by White in the following passage: "The libido has only two pathways open for it and these pathways lead in dia-

[11] *Ibid.*, 309. [12] *Ibid.*, 314 f. [13] *Ibid.*, 308.

[14] See my article, Instinct and the Psychoanalysts, above referred to.

[15] See, *e.g.*, E. Jones, *Treatment of the Neuroses*, 167; *The Psychoanalytic Review*, 8, 361–374; also Brill, Coriot, Jelliffe, etc., as cited in this article.

metrically opposite directions. One leads forward and upward, it is the pathway of constructive tendencies; the other leads backward and downward, it is the pathway of destructive tendencies. The former leads to fulfilment, life, immortality; the latter to dissatisfaction, failure, death. . . . The goal of immortality leads by the way of development, progress, evolution and is expressed by the conservation of personal life by means of health and the prolongation of personal life in children and of personal influence as expressed in a material way through the passing on of our personal qualities by way of the germ plasm or our spiritual tendencies by way of our works in our influence upon those about us, upon our own times, and upon the future by the record of our achievements that survive our individual existence. The death goal is by the way of the path of regression, the retracing of the path by which we have come, and leads to failure in the conservation of our individual existence, illness, invalidism (physical and mental) and to failure to hand on our influence either by way of the germ plasm or spiritually as a result of our works. These are the two pathways represented on the one hand by the drag back of our unconscious instinctive tendencies and on the other by those tendencies sublimated and applied to constructive conscious ends, the ambivalent goals of which are death and life motived respectively by fear and desire. This is the conflict, the path of opposites, along which somewhere, specific tendencies clash and cause that splitting of the psyche, so clearly seen in psychotics, which divides the energies of the individual and leaves him torn and broken upon the rocks of indecision, with his consciousness raised to an acuteness which is painful (fear, anxiety) in its terrific efforts to effect an adjustment."[16]

As long as the "libido" flows uninterruptedly into successful and satisfying action in the real world there is no conflict.[17] But if for any reason expression is blocked through the normal activity-channels, the "libido" flows backward within the individual himself, through the deepened channels of individual or racial past experiences, "reanimating old experiences." This is introversion of the "libido," and "disorder of the mind is the result."[18] This avenue of escape of the "libido" by means of introversion or regression may prove to be quite harmful. White regards it as the most characteristic element in dementia praecox.[19] "Perhaps the libido can get out of older levels but in doing so it offends the ideals which the individual has acquired in his upward strivings and so the result is illness."[20] This might properly be called the secondary conflict, the primary conflict being between two tendencies to action, one of which is inhibited on the plane of everyday reality and driven back upon older and more primitive avenues of expression. If these primitive modes of expression are in turn blocked, this time by ideals or values which censor the primitive impulses, a secondary conflict arises, and apparently, according to White, it is at this point that the true psycho-neurosis arises. The "libido" now attempts a third avenue of expression, that of subterfuge or symbolization or phantasy formation.[21] "The object of phantasy may be considered as twofold. The first, and less important object, I think, is the object of finding an outlet for the libido at an older level when faced with a situation to which adjustment is difficult. It is a way of letting off steam or, as the phrase goes, of emotional catharsis."[22] The dream may serve this function in some minor degree.[23] But "the more important function of phantasy is coupled with

[16] *Mechanisms of Character Formation*, 271f. See also his *Outlines of Psychiatry*, 9f.

[17] *Mechanisms of Character Formation*, 305.

[18] *Ibid.*, 285.

[19] *Ibid.*, 304. See also White, Some Considerations Bearing on the Diagnosis and Treatment of Dementia Praecox, *Psychoanalytic Review*, 8, 417–22.

[20] *Ibid.*, 305. [21] *Ibid.*, 305. [22] *Ibid.*, 305f. [23] *Ibid.*, 306.

its . . . function of portraying conflict, that is, its picturing of the two opposed tendencies that are battling for supremacy . . . The object of the symbolization of the conflict is, therefore, in general, to bring the whole matter into consciousness but in particular to bring that particular element into consciousness which is interfering with the progress of the individual."[24]

Thus it would appear that White, like Freud, regards the psychoneurosis as a substitute expression and as a symptom. But he does not maintain that it is dominated exclusively by the sex-impulses. Apparently it can arise from any type of repressed striving. We saw that Freud's ego or self-preservative tendencies, which produced the psychoneurosis by coming into conflict with the sex-impulses, in large measure really represent the censorship of the psychosocial environment, under the direction of which the former impulses are formed. White practically acknowledges this when he speaks of the conflict (which we have classed as secondary) as arising between regressive expression of the "libido" and "the ideals which the individual has acquired in his upward strivings."[25] These ideals could come only from his contact with the psychosocial environment, or the environment of institutions and social values. It is obvious that White is not so much interested in the structure of the psychoneurosis or phantasy as is Freud. He views it primarily from the standpoint of the diagnostician who is searching for a symptom which will enable him to discover the conflict and to remove the cause.

Jung has made considerable emendations of the theory of conflict-neurosis as held by Freud and his followers. He does not agree that the neurosis arises from some infantile sex-traumatism which recurs in the conduct of the individual and is based on the subconscious dispositions. All children, practically, undergo sex-shock, but only a few develop neurosis. Many of the stories of shock are invented by the patients,[26] probably as wishes rather than as facts. Jung does agree, however, that the neurosis is accompanied by an infantile fixation. "The aetiological secret of the neurosis, therefore, does not consist in the mere *existence* of infantile phantasies, but lies in the so-called *fixation*."[27] Again; "The psychological determination of a neurosis is only partly due to an early infantile predisposition; it is due to a certain actual cause as well."[28] The infantile phantasies merely determine the form and further development of the neurosis.[29] The neurosis is based on conflict and this conflict is primarily in the present.[30] "Whenever the libido, in the process of adaptation, meets an obstacle, an accumulation takes place which normally gives rise to an increased effort to overcome the obstacle. But if the obstacle seems to be insurmountable, and the individual renounces the overcoming of it, the stored-up libido makes a regression. In place of being employed in the increased effort, the libido now gives up the present task and returns to a former and more primitive way of adaptation."[31] The neurosis is the consequence of the overflow—is the overflow—of the stored-up energy or libido. "The psychological trouble in neurosis, and neurosis itself, can be considered as an act of adaptation that has failed."[32] "Even when we find perverted sexual phantasies of which we can prove the existence in childhood, we cannot consider them of

[24] *Ibid.*, 306f.
[25] *Ibid.*, 305. For further discussion of this censorship of the herd, see 326–329.
[26] Jung, *Analytical Psychology*, 227.
[27] *Ibid.*, 228f.
[28] *Ibid.*, 230, 234.
[29] *Ibid.*, 233; *Psychology of the Unconscious*, 463.
[30] *Psychology of the Unconscious*, 192; *Analytical Psychology*, 227–235.
[31] *Analytical Psychology*, 231f.
[32] *Analytical Psychology*, 234; *Psychology of the Unconscious*, 463.

aetiological significance. A neurosis . . . is not a primary phenomenon, based upon a perverted sexual disposition, but merely secondary, and a consequence of a failure to apply the stored-up libido in a suitable way."[33]

The neurosis stands, then, as a substitution in action or thinking for reality.[34] It is a form of illness arising from self-denial,[35] as Freud said. On this point Freud and Jung are fully agreed. It takes on the infantile form because the subconscious is full of stored experiences or memories which can be called upon to serve as channels for the overflow of substituted actions or thoughts when the hardness of reality blocks the way for the normal response of the conventional or expected kind.[36] The infantile experiences or memories are like a publisher's morgue, from which can be drawn almost any action-pattern to take care of the overflow of dammed-up energy. The same sort of substitution takes place in everday life. When we find some objective too difficult of attainment we often change our aim to something which we regard as more feasible. Ordinarily we do not consider this action pathological, for we select some other normal objective as a substitute; it is an exchange of a more difficult for a less difficult reality. It is only when the substituted act is not conventional, not a "reality," is in the nature of a phantasy and is shocking to other people, that we classify it as a psychoneurosis. There are also border-line cases, where we speak of the person making the substitution as "queer" rather than as strictly neurotic. Jung says: "He who has missed the happiness of woman's love falls back, as a substitute, upon some gushing friendship, upon masturbation, upon religiosity; should he be a neurotic he plunges still further back into the conditions of childhood which have never been quite forsaken . . . Every psychoanalysis carried out at all thoroughly shows this regression more or less plainly."[37] Thus the neurotic differs from the hypothetically normal person only in degree. He is an intensely disappointed person, while everyone is more or less disappointed. It is clear also that to both Jung and Freud the hall-mark of neurosis is the infantile fixation, although to Jung this fixation is not the cause of the neurosis. The neurosis usually occurs under great immediate stress, "at the moment when a new psychological adjustment, that is, a new adaptation, is demanded."[38] To the critic it may seem quite doubtful whether infantilism is a necessary correlate of neurosis.

Although Jung does not accept the Freudian view that neurosis is necessarily the result of sex-conflict and holds that the apparent sexual character of the neurosis is often assimilated to it along with its infantilism,[39] he does admit that the field of sex-conflict is in a high degree productive of the neurotic condition.[40] In this assertion and in the statement that the neurosis arises out of a present situation Jung, by implication, assigns great importance to the social environment in the production of the neurosis. In one connection the author seems to imply that conflict with the external environment does not tend to produce neurosis. "If it were an external difficulty, then, indeed, the libido would be violently dammed back, and would produce a flood of phantasies, which can best be designated as schemes, that is to say, plans as to how the obstacles would be overcome. They would be very concrete ideas of reality which seek to pave the way for solutions," but not substitute-phantasies aiming at the escape from reality by means of a neurosis which protects the sufferer from his environment.[41]

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[33] *Analytical Psychology*, 233f.
[34] *Psychology of the Unconscious*, 339, 432f., 460–5.
[35] *Ibid.*, 259 ff. [36] *Ibid.*, 421, 461–5.
[37] *Analytical Psychology*, 156.
[38] *Ibid.*, 229.
[39] *Analytical Psychology*, 227, 231, 277, etc.
[40] *Ibid.*, 367 ff.
[41] *Psychology of the Unconscious*, 196 f; *Analytical Psychology*, 297 f.

On the other hand, Jung specifically states the conflict in terms of the struggle of the inner impulses (largely the sexual, because this aspect of life is most repressed in our civilization[42]) and the pressures of the social environment. "Instinct thirsting for liberty thrusts itself up against the yielding barriers of morality: men are tempted, they desire and do not desire. And because they will not and cannot think out to its logical conclusion what it is they really desire, their erotic conflict is largely unconscious; whence comes neurosis."[43] He means to say here that repression from the social environment has driven the conflict into the subconscious, from which it emerges as a neurosis. He further adds that this conflict-neurosis is primarily concerned with the problem of repressed sex-expression. "Neurosis then is most intimately bound up with the problem of our times and represents an unsuccessful attempt of the individual to solve the general problem in his own person. Neurosis is a tearing in two of the *inner self*. For most men the reason of this cleavage is the fact that their conscious self desires to hold to its moral ideal, while the unconscious strives after the amoral ideal, steadfastly rejected by the conscious self."[44] Again he emphasizes the social nature of the conflict-neurosis: "We always find in a patient some conflict, which, at a particular point, is connected with the great problems of society; so that when the analysis has arrived at this point the apparently individual conflict is revealed as a universal conflict of the environment and the epoch. Neurosis is thus, strictly speaking, nothing but an individual attempt, however unsuccessful, at a solution of the general problem . . . The 'question' which troubles the patient is—whether you like it or not—the 'sexual' question, or more precisely, the problem of present-day *sexual morality*."[45]

It does not necessarily follow, however, that this conflict between our inner tendencies and the external social reality can have no issue except in neurosis. To be sure, the modern world is getting farther and farther away from the primitive world in which the two primal impulses—nutrition or self-preservation and reproduction or the race-preservative—find direct and uncensored satisfaction (at least, uncensored as far as public opinion is concerned). But along with the growth of opportunities for conflict there develops also an expansion of the "libido" or forms for energy expression. The "libido" (man's impulses) becomes more and more socialized. It develops from the primitive nutritive interest over to the relatively social sex-interest;[46] from this it expands, or is sublimated, into the religious interest,[47] which is at first very decidedly mingled with the sexual,[48] but finally comes to have largely a rational and refined emotional character;[49] and finally, at least in the minds of the more philosophic of our population, the "libido" (or impulses) is suffused largely with the spirit and data of science.[50]

Strictly speaking, perhaps, to be perfectly orthodox in our definitions, religion and science or philosophy might be described as neuroses, at least insofar as they contain infantile elements. Anyway, from the psychoanalytic standpoint, they are substitutions for primal satisfactions, but they are primarily substitute-interpretations of reality rather than substitutions for reality. However, it is difficult to distinguish between substitutions for reality and substitute-interpretations of reality. Possibly no definite distinction can be made in this connection. The expansion or socialization of the "libido" is in the nature of prophylaxis. When the conflict has already arisen and a neurosis is formed or in process of formation

[42] *Psychology of the Unconscious*, 460–2; *Analytical Psychology*, 374 f.
[43] *Analytical Psychology*, 369 f.
[44] *Ibid.*, 370.
[45] *Ibid.*, 374. Compare *Psychology of the Unconscious*, 258 ff.
[46] *Psychology of the Unconscious*, 146–9, 160 f.
[47] *Ibid.*, 258, 528. See also *Analytical Psychology*, 224.
[48] *Ibid.*, 256. [49] *Ibid.*, 257 ff. [50] *Ibid.*, 262.

cure may be effected by removal of the factor damming up the "libido."[51] Applied on a wide scale, this method might range in content all the way from a widespread education of all in the principles and facts of science to a return to savagery or to a higher animal level of existence, where there would be no artificial civilization to censor the native instincts of man. But this remedy is probably worse than the disease.[52] Jung warns against the attempt to "help the neurotic patient by freeing him from the demand made by civilization; we can only help him by inducing him to take an active part in the strenuous task of carrying on the development of civilization. The suffering which he undergoes in performing this duty takes the place of his neurosis."[53] This strenuous method can usually be avoided by sublimation, as an alternative to letting down the bars.[54] Sublimation is doing for the individual what has been or is being done for the race by the socialization and objectification of the "libido" or impulses through religion, art, philosophy and science. In a sense the neurosis is itself an attempt at self-cure,[55] a struggle to find a less burdensome solution to conflict, one which will work. Where the attempt is successful it is a cure; where it fails we have neurosis.

MacCurdy[56] accepts three fundamental instincts (ego, sex and herd) and maintains that there are four types of conflicts: those between ego and sex-tendencies, between the ego and the herd-tendencies, between the sex and the herd-tendencies, and between one type of sex-ambition and another.[57] Many writers on psychoanalysis do not allow for this last type of conflict. Freud includes it only when one type of sex "libido" has been assimilated to the ego-tendencies, as in the case of narcissism.[58] Serious conflicts occur only when the opposing tendencies are pretty evenly matched,[59] and all true conflicts must be between instincts, one of which must be repressed or transformed in its expression before the conflict can be removed.[60] The severest conflicts are between the sex and herd groups of instincts,[61] and the most marked psychoneuroses arise out of this type of conflict. "The next most important conflict is between the ego and herd groups,"[62] but the psychoneuroses arising hence are not particularly striking. The reason is that the individual adjusts to the demands of society because of the advantage he reaps from doing so. "When these (social or herd) conventions cramp individual ambition, the ego accepts the restriction because it gets a *quid pro quo*. This compensation takes the form of protection from danger, a share in material prosperity not achievable by the individual alone and, above all, a feeling of comfort derived from unity with the group that is a direct emotional expression of the herd instinct."[63] Furthermore, the herd does not repress all ego-tendencies. Consequently pathological expressions or psychoneuroses resulting from this type of conflict arise in only two possible situations. "One is where there is a constitutional predominance of ego tendencies such as seem to exist in epilepsy; the other is under exceptional circumstances when the ego is denied normal expression. These circumstances would be episodic and therefore produce only episodic abnormalities enduring only for the period of the emergency."[64] There does not seem to be much permanent conflict between the ego and sex-instincts, "for there is an alternation of attention to one type of stimulation or another, at least in so far as the pleasure aspects of the sex instincts

[51] *Analytical Psychology*, 232.
[52] *Ibid.*, 221. [53] *Ibid.*, 224 f. [54] *Ibid.*, 375 f. [55] *Ibid.*, 234,374.
[56] J. T. MacCurdy, Synthetic View of Ego, Herd and Sex Instincts, *Journal of Abnormal Psychology and Social Psychology*, 16, 1921, 249–268.
[57] *Ibid.*, 258.
[58] Freud, *General Introduction to Psychoanalysis*.
[59] MacCurdy, *op. cit.*, 259.
[60] *Ibid.*, 251. [61] *Ibid.*, 260. [62] *Ibid.* [63] *Ibid.*, 253.
[64] *Ibid.*, 254.

may conflict with other pleasures of the ego."[65] He further adds: "The probability is that important pathogenic conflicts would not result from the interaction of sex and ego instincts alone."[66]

The explanations offered by MacCurdy for the stronger conflicts between the herd and sex-instincts and the more pronounced psychoneuroses arising from these conflicts are interesting, but they omit the essential factor. Three explanatory factors are enumerated. First, "an important cause for continuous conflict between the sex and herd instincts is that sex is not a periodic function in the races of man we know about . . . A second and all-important point is that the channels for sex stimulation are, many of them, identical with the channels of social contact."[67] This is true of organic structure as well as of social intercourse. "The community of interest, the moral and physical propinquity which draw people into groups may draw two individuals into sex relationship. It is therefore difficult for one to receive a social stimulus without at the same time being subject to a sex stimulus. The latter falls, almost invariably, under social tabu, hence a constant conflict between the synchronously stimulated herd and sex instincts is inevitable."[68] Thirdly, the relatively freely expressed auto-erotic tendencies of childhood begin to be objectivated as the child grows more mature, and they must take cognizance of the social environment which is largely antagonistic to them. Hence conflict arises and phantasies and psychoneuroses result.[69] The strongest social censorship is directed against those sex-practices which inhibit marriage and propagation, such as inversion, perversion and incest. In fact, the writer thinks that there is an instinctive opposition to incest, because of the harmfulness of inbreeding.[70] On the other hand, adultery, prostitution and concubinage are lightly condemned because they do not conflict with the "fundamental urge for propogation."[71]

The thing which is constantly overlooked in this account of the superior strength of the sex-psychoneuroses, although it is literally calling for recognition from the context, is the environmental factor. As pointed out above, the sex-impulses contain a much larger instinctive element than the ego and herd-impulses, which are largely acquired. The herd-impulses are almost wholly acquired. Consequently, the sex-tendencies are too deeply rooted to give way easily under environmental pressures, and if repressed in one direction they break through in some other direction. If this variant outlet be through an activity which is not functional in a socially useful way, such as tics, hysterical paralysis, etc., it may be characterized as a neurosis. If the alternative expression is through unconventional or socially disapproved or "unreal" mental processes or ideas, it is called a psychosis. The ego-tendencies being more commonly instinctive than the herd-tendencies in the individual, but less so than the sex-tendencies, yield with a medium resistance to the demands of the social environment and produce weaker neuroses and psychoses. But since the herd-tendencies in the individual are mainly acquired from the pressures of the psychosocial environment, and are but little rooted instinctively, they find little occasion for conflict with the social environment and rarely produce neuroses and psychoses. In fact, these tendencies are often so weak that the constant pressure of large numbers of people upon our consciousness, making unbearable demands upon our nervous systems, produces psychoneurosis itself, this being one of the causes of nervous breakdown. The writer himself cites from Rivers' observations among the Melanesians a type of insanity which we might call herd-fear,[72] and this is not unknown among civilized peoples.

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[65] *Ibid.*, 252. [66] *Ibid.* [67] *Ibid.*, 256.
[68] *Ibid.*, 256 f. [69] *Ibid.*, 257 f. [70] *Ibid.*, 255. [71] *Ibid.*, 254 f.
[72] *Ibid.*, 259.

The fact is that MacCurdy, like all the psychoanalysts and psychiatrists with a psychoanalytic bent, is confused as to the basis of the conflicts. They generally assume that the conflict is between the so-called instinctive or subjective tendencies and impulses, while, as a matter of fact, it is only secondarily so. This inner conflict is between two sets of subjective neuropsychic processes which can be brought equally well under the control of consciousness or thought and resolved into some sort of psychic unity through compromise or dominance of neural sets. There is no need for the generation of phantasies or a substitute-method of resolving the conflict, because the conflict is "thought" out of existence. In the process of thinking the conflict disappears. There is no distinction between reality and phantasm as far as the subjective neuropsychic processes are concerned. This distinction refers to the environmentally determined facts of the character and availability and immanence of the stimuli and the responses which are connected through the central processes of the stimulus-response arc, where the thinking occurs. Consequently there can be no psychoneuroses arising out of inner conflict of mental processes. Any conflict here must soon be resolved into a unitary attitude or concept. We see things as wholes, even if we are compelled to select our stimuli and control our responses in order to do so.

The real and primary, the stubborn conflicts are between subjective mental or neuropsychic processes and the outside world or environment. The mind cannot control the outside world and order it at will as it can the mental processes. It cannot whip environmental factors into an objective unity in conformity to inner psychic organization. Consequently, when the conflict between external reality and inner mental processes or complexes is sufficiently marked, the mind, in its struggle for inner unity, resorts to the device of selecting external stimuli and the perversion of perception and ideas. This is one of the most common phenomena of psychic life, but it ordinarily takes place on a limited scale, because the conflict between the inner psychic organization and the external reality is not usually especially marked. All of us see things from our own subjective angles. But when we grossly misinterpret external phenomena to fit our subjective needs or desires we regard it as abnormal. It is then that the fact of the personal equation in interpretation reaches the dignity of phantasy, and is classed as neurosis or psychosis. If we act out our phantasy, directly or symbolically, we suffer from a neurosis. Such abnormal mental processes occur because, on the one hand, the environment will not yield to the powers of mental control which the subject possesses and, on the other hand, the mental organization itself cannot immediately adapt itself to the pressures of the external environment.

Freud almost hit upon this principle in his theory of the conflict between the principle of pleasure and the principle of fact,[73] which is referred to by other writers as the conflict of the pleasure-motive and the reality-motive.[74] He was prevented from making the application because of his lack of appreciation of the functional significance of the environment in the conflict situation. Also, possibly, the regnant metaphysical notions of reality, which tend to make it subjective instead of objective, neglecting environmental analysis, unconsciously influenced his thought. Anyway, he missed the scientific explanation, based on objective analysis, and contented himself with a metaphysical and merely verbal theory instead. In this the other psychoanalysts, following the metaphysical and mystical tradition, have persevered. Thus it happens that MacCurdy, like the others, confuses conflict between the internal psychic processes and the objective environment with conflict between the sex and ego, inner or psychic

[73] *General Introduction to Psychoanalysis*, 309.

[74] White, *Mechanisms of Character Formation*, 56, 71; Lay, *Man's Unconscious Conflict*, 85, 93; *Man's Unconscious Passion*, 18; etc.

tendencies on the one hand and the inner, psychic herd-regarding tendencies on the other. They do not distinguish the herd-complex (miscalled herd-instinct) from the herd itself. The former is the acquired product of the impingement of the latter upon the consciousness of the individual. The sex, ego and herd-complexes all belong to the same order of phenomena, and may severally be in conflict with the herd or social environment. This is the significant type of conflict, while the conflict of the first two complexes with the last is relatively unimportant and easily resolved. This confusion of the psychoanalysts is all the more remarkable when it is remembered that their data constantly call attention to the persistence of the environmental pressures. Being wedded to their uncritical use of the instinct-theory or phantasy, they are unable properly to evaluate objective reality in the form of the environment.

MacCurdy recognizes three immediate factors in the production of the psychoneuroses. The normal balance of the mind is maintained through sublimations. But "when the energy supply is low (owing to such organic factors as illness or strain) sublimations are weakened and instincts of the sex or ego group express themselves more directly. This is the principle of regression. Another situation is that of environmental stress. When circumstances make the maintenance of sublimations difficult or impossible the same mechanism comes into play . . . Any environmental event, then, which weakens such sublimations can lead to regression."[75] His explanation of how the psychoneurosis so generally takes on a sex-character, although it may not be sexual in its ultimate origin, is interesting. "Such events may be business or political disappointments which may have little to do with sex. When these conscious activities fail to give satisfaction the process of regression leads to an attempt at unconscious satisfaction. Since the unconscious is constituted largely of egoistic sex cravings the immediate determinants of the ensuing symptoms may be sexual, without the general cause of the disease having anything to do with sex.[76] This conclusion of course assumes the truth of the second term in the syllogism, namely that the subconscious content is repressed sexuality, which is not necessarily true. May not a better explanation be found in the fact that the sex-processes, being largely instinctively based, have deep neuropsychic channels, which fact facilitates the association or overflow of all sorts of impulses into these processes resulting in their mixture or substitution? He continues: "A third pathogenic factor (in the production of psychoneuroses) is the direct stimulation from without of an unconscious tendency. For instance, if the rival of the unconscious Oedipus drama dies the way is left open for autistic indulgence of this primary ambition. This may lead to an inflation of unconscious sex cravings."[77]

Jelliffe[78] apparently adopts a somewhat different basis for conflict. Although, like Freud, he recognizes two great instincts (reproduction and self-preservation) and two only,[79] all others being merely "combinations, amplifications or partial elaborations of these,"[80] he recognizes a great many separate "libidos,"[81] which are individualistic or self-seeking in early childhood[82] and are united by a process of discipline and fusion under the direction of the social environment[83] after the age of five.[84] Curiously enough, he does not suppose these several "libidos" to be founded on corresponding instincts, bearing the same names, although he clearly defines his "libidos" as energy striving for expression towards certain ends.[85] In this he is, of course, correct; for each "libido" represents for him a func-

[75] *Op. cit.*, 260 f. [76] *Ibid.*, 261. [77] *Ibid.*

[78] S. E. Jelliffe, *The Technique of Psychoanalysis.*

[79] *Ibid.*, 66. [80] *Ibid.*, 67. [81] *Ibid.*, 40–48, 62 f., 132.

[82] *Ibid.*, 41. [83] *Ibid.*, 42, 44, 53, 56. [84] *Ibid.*, 67. [85] *Ibid.*, 40, 44, 66.

tioning group of organic processes, such as respiratory, nutritive, pelvic, sensory, etc.,[86] which are grouped together not so much because they are structural unities in action as because they are recognized by the human mind as contributing in their several ways to the same sorts of environmental adjustment. That is, their unity is more external or objective than internal and subjective. However, the two so-called instincts which he does recognize[87] are defined in this same loose abstract and environmental manner.

It is difficult to determine just where the conflict occurs according to Jelliffe. Apparently it has abundant opportunity to arise during the first five years among or between the various individual or partial "libidos," and probably it does start here. But in the course of time the social environment, at first in the form of the family,[88] and later in that of society at large, represses these separate ego or "libido" tendencies and welds them into a sort of unity for the sake of self-preservation of the whole organism.[89] Elsewhere, however, his statement of the opposing tendencies in life takes on a much more mystical and metaphysical form. He speaks of "concrete immortality" as "the goal of the libido striving, which is reached either directly and foremostly through the birth of children or secondarily through building up of social structures and all that goes to make life worth living."[90] Evidently he identifies the general "libido," as distinguished from the partial "libidos" mentioned above, with the successful prosecution of the two general instinctive tendencies, reproduction and self-preservation. That being the case, serious conflict cannot arise between these two instincts, at least after the age of five. We are not left without a statement of the conflict-process, however. In equally metaphysical language he informs us that "the direct opposite of personal immortality is death," and that fear is its symbol.[91] Are we to conclude, therefore, that conflict arises between these two concepts or between the emotions of hope and fear which attach to them? Other passages which have been cited from the text would seem to imply that the conflict is between the two general instinctive tendencies, which are basic to the general "libido" and the various partial "libidos" on the one hand, and the restraining and moulding environment on the other. In this way the conflict has really a double origin. It arises partly from the internecine warfare of partial "libido" trends before they are integrated by the environment into a unified personality, and partly from the struggle of this resulting personality with the environment. In the latter case the conflict is between the "libido" and the environment, and in the former it may be either between the separate "libidos" or between any one or all of the partial "libidos" and the environment. If there is wanting a concrete and concise statement of this view by the author it is probably due to the fact, previously adverted to, that the psychoanalysts, absorbed in the popular instinct-interpretation, have not yet had the environmental factor definitely and adequately called to their attention.

The rest of Jelliffe's theory of conflict-neurosis is quite according to type. When the "libido" fails to secure adequate expression in action it turns to thought instead, and when this takes on the form of phantasy of a marked character with attendant unconventional or uneconomic and unreal activities, as viewed from the standpoint of an efficient life adjustment, we have the psychoneurosis.[92] Since the social environment is apparently the great censor, antisocial tendencies are especially liable to find substitute expression in phantasy and psychoneurosis.[93]

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[86] *Ibid.*, 40. [87] *Ibid.*, 66.

[88] *Ibid.*, 53, 56. [89] *Ibid.*, 47 f. [90] *Ibid.*, 67.

[91] *Ibid.*, 67 ff. *Cf.* White, *Mechanisms of Character Formation*, 271 ff.

[92] *Ibid.*, 41, 45 f., 54, 64 f., 119, 137, 139, 142. [93] *Ibid.*, 45.

Brill[94] takes the conventional point of view regarding the origin of the conflict-neurosis which we have found in the discussions earlier set forth, and to these he adds some interesting applications or extensions of the theory. He says that the individual suffers from strangulated emotions; "certain feelings and ideas which we would like to give vent to, but cannot. . . . They are finally pushed into the unconscious."[95] Or, on the other hand, "an individual experiences a profound emotional shock and cannot give it emotional expression; it remains in a repressed condition."[96] These repressed or strangulated emotions, associated with painful experiences in many cases, form a complex which is brought to the surface only by some association. This conflict, from which the neurosis arises, may have its origin in any situation which represses our instinctive tendencies or emotions, but it centers primarily around the inhibition of the sex-instincts and emotions.[97] Brill even goes as far as to sanction "Freud's dictum that no neurosis is possible in a normal sex life,"[98] and he maintains that this statement is true also of the more radical functional disturbance known as psychosis.[99]

The neurosis, according to Brill, is to be recognized by the neurotic symptom.[100] This is some substitute-expression which is symbolic, in the mind of the subject, of the thing denied expression by the psychic censor.[101] These substitute-symptoms may be drawn from any source, but they are taken particularly from the vast treasure-house of forgotten childhood experiences.[102] These, although repressed and forgotten, are imperishable and may be brought out at any time to do service in this connection through the principle of association.[103] The fact that the childhood experience is so often used as the basis of the symbolic symptom at first led the psychoanalysts to believe that the neurosis was the product of some infantile or early shock. But the tendency among the psychoanalysts at the present time is to attribute the conflict primarily to sex-repression. Civilization has removed the hunger conflict, the other potential source of the neurosis, so that neurosis does not, at least with any frequency, any longer arise from the maloperation of the instinct of nutrition.[104] But, on the other hand, civilization has only served to increase the improper functioning of the sex-instinct by imposing still further restrictions upon its expressions.[105] Hence the frequency of neurosis arising from this source. Even where neurosis arises from some non-sexual cause, such as the loss of a child or failure in some undertaking, apparently it could have been prevented if sex-love had not been lacking as a salve or substitute for the other loss.[106] Thus, perhaps, we may infer that sex-love is almost, if not quite, a universal panacea for ills of the spirit. This view is in harmony with the statement earlier cited to the effect that there can be no neurosis or psychosis with a normal (or satisfied) sex life.[107]

Many of the conflicts giving rise to neurosis appear in connection with unfortunate love-affairs. Others arise from the lack of opportunity of the sex-powers to function. This is especially true of women, who are more neurotic than men. "In our Anglo-Saxon communities, they (our sex impulses) have not even the aesthetic and social outlet, because of the too great separation of the sexes. The result is that, owing to the matrimonial difficulties and the two-children system, the women especially who have not been able to express themselves adequately for centuries and whose lot is growing harder and harder in our civilization, suffer from a marked need of love. If you find a woman depressed and out of sorts with herself

[94] A. A. Brill, *Fundamental Conceptions of Psychoanalysis*, 1921.
[95] *Ibid.*, 13. [96] *Ibid.*, 14. [97] *Ibid.*, 22. [98] *Ibid.*, 29. [99] *Ibid.*
[100] *Ibid.*, ch. 2. [101] *Ibid.*, 38–40, 44, 63. [102] *Ibid.*, 25.
[103] *Ibid.*, 20, 181. [104] *Ibid.*, 32. [105] *Ibid.*, 30, 32, 291.
[106] *Ibid.*, 31. [107] *Ibid.*, 185 ff.

and weeping, who cannot tell you what is ailing her, you may safely conclude that she is craving for love; give her the necessary love outlet through happy marriage and children, and she will have no more crying spells."[108] Much of this conflict and its consequent repression may be symbolized as a conflict between the past and the present, on the one hand, and between the infantile nature and the adult nature, on the other hand. In the first instance our natures, instincts, were selected to fit a natural or pre-civilized world and these impulses persist into the civilized world and come into conflict with our conventions and institutions.[109] "Centuries of civilization have left their mark upon us, and we must now live accordingly . . . There is therefore a constant struggle; no one likes to submit to the repression that the inhibitory aspects of civilization demand; no one finds it easy to repress the primitive impulses."[110] Thus the individual learns the meaning of "no" from infancy. "His life . . . is one long struggle, one bitter revolt."[111] In much the same way the primitive desires of childhood, which may be tolerated before the age of accountability, will be repressed by the social censor when the individual reaches or approaches adulthood.[112] It is significant, Brill holds, that the child's dreams become more difficult to analyze after the age of five. There is no difficulty before that age.[113] It is then that the censorship begins to operate by virtue of the child's growing perception of the conventional. Also "it is noteworthy that it [the child] develops also at this time a sense of humor."[114]

The censorship over repressed impulses and ideas is exercised by the conscious.[115] "The mind is always protecting us from pain by pushing whatever is disagreeable and unpleasant into the unconscious."[116] This fact affects the conventional or popular theory of memory: "thus what we generally look upon as forgetting is not that at all; certain things are merely pushed into the unconsciousness, because of something unpleasant associated with them; we are not aware of them consciously and we naturally presume we have forgotten them."[117] That which sets the standards of the censorship is one's understanding of what is conventional and of what is forbidden.[118] "The psychic censor, as the term is used in our work, is nothing but the inhibitions imposed upon the individual by society, and once established, operates all the time, whether we are awake or asleep."[119] The censor can be avoided only by subterfuge, and of this nature is the dream.[120] But it is not alone in dreams that we find this attempt at avoidance by subterfuge. Our opinions, our every day actions, attitudes, alliances and allegiances indicate it. Thus day-dreaming and the artificial dream show the same tendency.[121] Any form of make-believe or wish betrays the same type of stratagem. Lying and hypocrisy represent veiled wishes and strivings against the censor.[122] Fairy tales, [123] which are a form of lying, and art,[124] which may be either subterfuge or sublimation or both, also belong to the same general category. Modern art is decidedly auto-erotic[125] and the productions of the futurists and other radical artists resemble closely those of insane people.[126] The expression of such art—painting, poetry, or otherwise—escapes the social censor by taking refuge in a theory of individualism or self-gratification (art for art's sake) which denies the validity of the social censor altogether. It is nihilism.[127]

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[108] *Ibid.*, 29 f. [109] *Ibid.*, 161. [110] *Ibid.*, 161 f. [111] *Ibid.*, 162.
[112] *Ibid.*, 161 ff., 207 ff. [113] *Ibid.*, 164.
[114] *Ibid.* [115] *Ibid.*, 16, 51, 75, 171. [116] *Ibid.*, 51. [117] *Ibid.*, 75.
[118] *Ibid.*, 171. [119] *Ibid.*, 225, note. [120] *Ibid.*, 23, chs. 6–9.
[121] *Ibid.*, 195. [122] *Ibid.*, 195, 200–208. [123] *Ibid.*, 296, 300, 313.
[124] *Ibid.*, 309 ff. [125] *Ibid.*, 312. [126] *Ibid.*
[127] *Ibid.*, 303–12.

The neurosis itself arises from an inability to meet reality.[128] Not all such failure results in neurosis, for many people successfully adjust themselves to substitute-expression without serious functional emotional disturbances.[129] They forget their former loves or desires and become wedded to new ones.[130] In youth this is relatively easy, although one may pay the penalty later in a period of deprivation by having the old experiences, or some symbol of them, become neurotic symptoms. In later life, when the neural organization becomes more set and one's ideals have less resiliency, it is more difficult to find and accept substitutes. Where the strain is too great, the neurosis appears. That is, the repressed complexes refuse to remain under, in the unconscious. They burst through in the form of a substitute-idea or activity which has become associated with the thing desired. The symptom is an outlet, a safety-valve,[131] but it may be more harmful than the act or idea for which it is substituted. The symptom may be a pain, an ecstasy, a dual personality, a fixed idea, or an unregulated enthusiasm or aversion. In the more severe forms of illusion the neurosis is called a psychosis.[132] Some substitutions are sublimations.[133] Others are self-deceptive revolts against repression or censorship. Some individuals cannot solve their adjustment-problems by either method; "they are men and women who could not learn to accept the facts, who could not 'forget.' Instead, they tore themselves away from reality altogether, and realized their wish in their own way, thus solving the problem."[134] They are the insane. Dementia praecox is an extreme example of the inability to face reality.[135]

The theory of the conflict-neurosis set forth by Adler[136] is essentially unique. He starts with a metaphysical concept of a native "impulsion toward a maximation of the masculine ego-consciousness" or male dominance,[137] with a complementary tendency in women which is very similar to the more general but equally metaphysical infantile "will to omnipotence" and "search for lost omnipotence" of other psychoanalysts and psychiatrists.[138] As long as all the organs, physical and mental, are functioning properly, action runs smoothly and there is no psychic difficulty. But if some organ proves to be inferior, success in the primary end of the striving is rendered difficult or impossible and a feeling of inferiority, the inferiority complex, arises.[139] Two ways are open in the struggle to overcome this feeling of inferiority. One method is to build up the efficiency of the inferior organ, which we sometimes do until it is more capable than some of those organs which possessed a better start. Or compensatory development of other organs or of economy of organization may be undertaken with favorable results. But if both of these methods fail the subject may substitute phantasy for achievement.[140] In its severer forms this phantasy becomes an acute psychoneurosis which the subject exploits as a means to gain the ends which his organ-inferiority renders it difficult for him to attain.[141]

Doubtless this theory does explain many cases of psychoneurosis, especially those which arise from maladjustments due to the partial in-

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[128]*Ibid.*, 37, 40, 160 f., 296.
[129]*Ibid.*, 40 ff. [130]*Ibid.*, 16, 47, 75. [131]*Ibid.*, 35.
[132]*Ibid.*, 26 ff. [133]*Ibid.*, 314, 320 ff. [134]*Ibid.*, 40.
[135]*Ibid.*, 28 f., 191, 253–261, 286.
[136]A. Adler, *The Neurotic Constitution.*
[137]*Ibid.*, 76.
[138]See White, *Mechanisms of Character Formation*, 177 ff.; *Outlines of Psychiatry*, 8 ff.; Ferenczi, *Contributions to Psychoanalysis*, 186–196; Lay, *Man's Unconscious Passion*, 184–8.
[139]*Ibid.*, ch. 1. [140]*Ibid.*, 56. [141]*Ibid.*, 96, 115–117.

capacity of the subject. But there are many other cases which are due to the uncompensated hardness of the environment, which bears more heavily upon some than upon others. Environmental pressures are by no means uniform, and some persons with strong organic equipment must fight a losing struggle because they are "unfortunate," while often weaklings have things made easy for them because of a fortunate environment. This is a phase of the subject to which Adler has not given adequate attention. It is interesting to note, however, that he conceives the struggle or conflict to be between the organism, with emphasis upon the weak organ, and the environment, and not between two instinctive tendencies. It is in this sense that his theory of the conflict-neurosis may be called an environmental theory.[142] On the other hand, it is decidedly subjective, for it derives the psychoneurosis from an attempt to compensate a state of consciousness, the feeling of inferiority, instead of deriving it from an unreal and self-deceptive (and other-deceptive) attempt at establishing an effective adjustment to the external environment. Its subjectivity consists in its remoteness and indirectness. It errs in opposing that there must be a recognition of inferiority in order that a false technique of adjustment, that is, a psychoneurosis, may develop.

Tansley's view of the basis of conflict is more explicit and dependable than those previously stated. "Mental conflict . . . is the struggle which takes place between two complexes whose conations would lead to incompatible actions."[143] He does not fully agree with Trotter that the impulses must necessarily have instinct behind them,[144] but holds that "the major conflicts are always between complexes which have the force of the great primitive instincts at their back."[145] Of minor conflicts he says: "Minor conflicts may occur between any two complexes of sufficiently strong affect whose conations are incompatible, but they are of little importance in the general mental life."[146] Speaking of the kinds of social correlations of conflict, he asserts that "the classical examples of major conflict are those between sex and religion, between sex and morality, between patriotism and the family, religion and the family, and so on."[147] These types of conflict are to be met with constantly in the novel and drama as well as in real life. "All these may be generalized as conflict between the strongest individual instinct, that of sex, and that other great instinct which rivals sex in strength and significance, the instinct attaching to membership in the herd."[148] Here evidently he has unintentionally substituted instinct for complex, which earlier he stated to be the basis of conflict. Unlike Freud and some other psychoanalysts, he believes that conflicts may arise within the domain of the same instinct. "In many cases a major conflict may take place between differentiated and segregated complexes derived from one great primitive instinct, for instance the conflict between religion and patriotism, which in some forms at any rate represent different aspects of herd instinct, conflicting loyalties to two different 'herds'."[149]

According to Tansley, conflicts may be disposed of in three ways.[150] In mild cases the weaker complex may be inhibited by the stronger, without serious consequences. In more serious cases, where one impulse cannot be inhibited by another but comes into action in spite of opposition, the resulting action may be harmonized by means of a rationalization. This is called a *defence reaction* and usually has some reference to the social en-

[142] *Ibid.*, 117 ff.

[143] *The New Psychology and Its Relation to Life*, 101.

[144] Cf. Trotter, *Instincts of the Herd in Peace and War*, 82.

[145] *Op. cit.*, 101. [146] *Ibid.* [147] *Ibid.* [148] *Ibid.* [149] *Ibid.*

[150] *Ibid.*, 102–5.

vironment. Where these two methods cannot remove the conflict from consciousness a third and more drastic method, that of repression, may be called into service. For a longer or shorter time this last method may prove successful, but in the end the repressed tendency may secure "expression in indirect, symbolic, and often curiously distorted forms[151] . . . It cannot be kept permanently quiet in the unconscious, and the imprisoned libido, sooner or later, breaks out to the surface with violence, generally as a result of the accidental disturbance of the precarious balance which has been initiated and maintained by the repression."[152] Here we find the origin of the psychoneurosis. The results of the repression may be "disastrous to the mental health of the patient. Such repression often gives rise to hysteria, which may take the form of a somnambulism or other hysterical symptom, or may bring about a more complete rupture of the self, as in cases of double or multiple personality, or even a definite type of insanity."[153]

Several typical psychoanalytic theories of the conflict-origin of neurosis have now been reviewed briefly and the more pertinent criticisms of these views have been made in connection with their exposition. We may turn now, by way of summary, to a statement of some general conclusions to be drawn from this discussion. In the first place, some psychologists and neuropsychiatrists would object to the use of the theory of conflict in explaining the origin of psychoneuroses and would employ some other concept, such, for example, as redintegration.[154] However, whatever the value of other concepts, it seems to be pretty well established that conflict, especially if we take account of its objective social as well as its subjective aspects, does play an important part in the functional neuroses. It is precisely at those points where strain is greatest and most intolerable that normal functioning of the mind breaks down and the psychoneuroses appear.

A second point to be noted is that the assumption regarding the content of the mental processes in conflict is erroneous. Complexes are miscalled instincts.[156] The so-called sex or reproductive, ego or self-preservative, and herd instincts are not inherited unit action-patterns, but are abstract value-concepts or classifications of activities according to similarity of end or aim which have no structural similarity or organic organization and unity. The so-called herd-instinct or herd-complex has the least truly instinctive content, the ego or self-preservative complex somewhat more, and the sex-complex most instinctive content of all. In all of these complexes environmentally produced habit dominates over instinct in the organization of action. Especially the sex or reproductive and the ego-complexes have large bodies of traditional and customary organization behind them, moulded by institutional pressures, which give the organiza-

[151] *Ibid.*, 107. [152] *Ibid.*, 112.

[153] *Ibid.*, 111. Cf. Janet, *The Major Symptoms of Hysteria.*

[154] Cf. H. L. Hollingworth, *The Psychology of Functional Neuroses.*

[156] Cf. Bernard, Instincts and the Psychoanalysts, above referred to.

tion of their activity-correlates a degree of stability and definiteness which often clothes them with the deceptive appearance of an inherited unity of organization. The child grows so gradually into conformity with these historic action-patterns that their acquisition by him is not obvious. This has done much to foster the error of belief in their instinctive character. The action-patterns for sex-expression are closely standardized around marriage, prostitution and certain other customary, although disapproved, sex-practices. The ego-tendencies likewise are grouped around certain customary and conventional methods of property-holding, self-protection, self-display, eating, and the like, as well as the consciousness of values of relationships and activities for the self, or self-consciousness. The self-consciousness is clearly acquired, growing up as a function of the adjustments of the human organism to its environment, in which process these adjustments are evaluated by the organism's consciousness.

The herd-complex consists of the consciousness and attitudes of the individual with respect to the social environment, and this consciousness grows up as a complement of the self-consciousness. The earlier analogues of the "herd-instinct" were the "religious" and "moral" instincts, which are now gone out of fashion among the psychologists. These last flourished in an age when thinking was theological or metaphysical,[157] that is, when social relationships were interpreted in terms of traditional dogmas and when morals were largely determined by ritualistic or customary conformity and conscience could be conceived of as an inherited entity. They were concepts of a lower stage of the development of the social consciousness. Their disappearance as instinct-categories is contemporaneous with the rise of the "herd-instinct" concept, which is merely a more enlightened substitute for them. The "herd-instinct" concept is more flexible; it corresponds to the scientific interpretation of social organization and control as proceeding from the dynamics of the motives and adjustments of people living together. It places the emphasis upon adjustment and organization of ends and methods of people living in the present, and discounts the traditional and customary convention and the externality of control which are characteristic of the theological and the metaphysical interpretations. But the "herd-instinct" is no more a reality than the "moral" and "religious" instincts which it displaced with a change in the fashion of thinking. It, like them and also like the so-called reproductive and self-preservative instincts, but even more than they, is an acquired complex, an abstract evaluation of actions or concepts related to the same perceived ends.

[157]Bernard, Religion and Theology, *Monist*, 1922, 61–88.

Thirdly, in enumerating the sources of conflict, the herd-complex and the immediate pressures of the environment are often confused. The herd-complex is the individual's more or less standardized conception of social values. It is what he thinks of society and of social rights and obligations.[158] It is true that this complex may come into conflict with another complex or, perhaps we might better say, that activities or ideas related to this and some other complex may come into conflict. But the strongest conflicts can scarcely be between two psychic complexes or processes pertaining to them. The chances for conscious integration and adjustment are too great where both complexes are liable to the same conscious supervision and control. The most serious conflicts arise between one's habits or complexes and an unyielding environment which cannot be controlled from within. It is an external thing, in a measure unexpected and unadjusted, the organization of which must be either transformed by the attack of consciousness upon it or it will transform consciousness and rebuild the complexes. Or, if neither of these comparatively normal results can be brought about, the consciousness will develop illusions and hallucinations or some other abnormal and unreal adjustments to the environment, which constitute psychoneuroses. Almost without exception the psychoanalysts confuse the herd-complex or "instinct" with the environment, and thus vitiate the accuracy of their interpretation.

In the fourth place, the complexity and strain of modern life, built around a vast industrial and scientific technique as it is, has intensified the conscious analysis of objects, both of the self and the environment, by the individual intellect. As a consequence both the ego and the social alter are now much better defined than they have ever been in the past, although there have long existed social and self concepts in abundance. Our attention has already been called to the institutionalized forms of self-expression and to the social "instincts" of religion and morality as examples of these. This modern accentuation of self and social consciousness, with a consequent closer definition of the content of these two poles of consciousness, has intensified the conflict which may arise between the corresponding complexes or these complexes and the sex-complex, on the one hand, and these complexes and the environment on the other. The vividness and definiteness of consciousness in these complexes makes possible the drawing of closer distinctions between tendencies to action with a consequent intensification of conflict. The growth in volume of the psychoneuroses bears testimony to this fact.

[158] Jelliffe approaches this interpretation, but does not formulate it, in his treatment of the welding of the conflicting egoistic partial "libidos" into an integrated personality under the influence of the social environment. *Op. cit.*, 48 ff.

Fifthly, as a corollary of this statement it may be proper to repeat Freud's statement that man only is capable of such intensity of conflict as is here implied and that only man has psychoneuroses.[159] If this is not absolutely true of some of the animals nearest to man in intelligence, it is relatively and essentially correct. The reason for this is, of course, that only man is capable of an effective conceptualization of environment and environmental forces. Therefore only he can develop that bipolar highly defined and intensified consciousness of self and society which makes it possible to transfer the mere physical opposition of individual and environment to the internal and acute psychic conflict.

Sixthly, the need for a rational analysis and organization of the external environment becomes obvious in the light of what has been said. A close analysis of the environment is necessary as a means to its classification and control. Organization of the environment will be possible in the degree to which we are able to define it, although complete organization and control, even with the most extensive applications of science, will not be ultimately feasible. Adaptation to environment is as imperative for the avoidance of extreme conflict as is control of environment. The psychoneurosis is itself in effect an illusion of organization and control of the environment when actual control has not proved possible.

Finally, this organization of the environment should in a large measure properly take the form of the provision of substitute-channels for expression of the energy of metabolism. As a matter of fact, but the smaller portion of our energies is attached by inheritance to instinctive channels of expression.[160] The surest way of avoiding disastrous conflicts which produce psychoneuroses is so to organize the environment as to provide channels of expression which increase the adaptation between the individual and his environment rather than foster the maladaptation or conflict aspects. This can be accomplished only by means of a thorough analysis of both the individual and the environment, especially of the psychosocial environment. This calls not only for the careful development of the science of psychology but of a science of sociology as well. Both are indispensable to the end desired.

[159] *General Introduction to Psychoanalysis*, 358. Of like significance is Brill's contention that neurotics constitute a high class of persons. *Op. cit.*, 25–7.

[160] Tansley, *op. cit.*, 84.

THE PERCEPTIONS OF LIQUIDITY, SEMI-LIQUIDITY AND SOLIDITY

By ALICE H. SULLIVAN, University of Illinois

The purpose of this study was threefold: (*a*) to compare the perceptions of liquidity and solidity, in order to find what conditions are peculiar to each of the two perceptions; (*b*) to compare the perceptions of liquidity and semi-liquidity, in order to discover the conditions peculiar to the perception of semi-liquidity; and (*c*) to find how many separate and distinct perceptions are included under the term 'liquidity'. Since there was nothing in the literature on touch blends to aid us in answering these questions except Bentley's work on the perception of liquidity[1], it was necessary to have recourse to experiment.[2]

The method of experimentation was as follows: the *O*, blindfolded, sat with his right arm supported by a raised arm-rest, with the middle finger extending down through a hole in the end of the arm-rest. After a 'ready' signal, *E* slowly raised, by means of a pulley, a small beaker containing liquid or other substance until *O*'s finger was immersed to the first joint. *O* then reported (*a*) upon the sensory components of the experience, (*b*) upon the pattern or arrangement of the sensations, and (*c*) upon the meaning of the experience. In order to avoid any clue to the nature of the stimulus, the finger was always wiped after every stimulation whether the stimulus was wet or dry. There were 8 *O*s, some of whom observed in all the experiments and some only in part.[3]

Experiment I.: Perceptions of Liquidity and Solidity Compared

In the first series of experiments, we compared the perceptions of liquidity and solidity. Water, sand, meal, flour, cornstarch, and cotton comprised the stimuli. The dry substances were sometimes cooled or warmed, and sometimes left at room temperature.

Table I. shows numerically the results of analysing the perceptions resulting when dry stimuli were used. It does not include the experiments in which water was used as stimulus because the dry substances, being sometimes warmed or cooled,

[1] M. Bentley, this JOURNAL, 1900, 9, 414.

[2] See references to studies on touch blends at end of this paper.

[3] The observers were: Professor M. Bentley, Dr. P. T. Young, and the writer, all members of the department of psychology, and Misses M. Bales, C. Blackman, O. Nilson, and D. Whitney, and Messrs. W. Lutyens and R. Newcomer, all seniors and major students in psychology.

furnished the conditions for perceptions of liquidity as well as solidity. Consequently, the comparison of the two perceptions was made on the basis of the experiments with dry substances.[4] The second column of the Table indicates the number of experiments; the third column shows the number of times the analysis revealed both pressure and temperature; the fourth column, the number of times the analysis revealed only pressure; the fifth column, the number of perceptions of liquidity or semi-liquidity experienced; and the last column, the number of perceptions of solidity experienced. In most cases columns 3 and 5 and columns 4 and 6 refer to the same experiences.

TABLE I.

Comparison of Perceptions of Liquidity and Solidity

Obs.	No. of Exps.	Pres. Temp.	Pres. alone	Percep. of Liquidity	Percep. of Solidity
Ba	5	1	4	1	4
Bl	1	0	1	0	1
L	6	3	3	1	5
Ne	3	2	1	2	1
S	25	11	14	10	15
W	7	2	5	0	7
Y	22	13	9	9	13
Total	69	32	37	23	46

The Table shows that, generally speaking, perceptions of liquidity are made up of blends of pressure and temperature, while perceptions of solidity are made up of patterns of pressure alone. *E.g.*, Ba reported one perception of liquidity and one case in which both temperature and pressure were present in the analysis, while she reported 4 perceptions of solidity and 4 cases in which the analysis showed a pressure existing alone. Out of a total of 69 experiments, there were 23 perceptions of liquidity and 32 cases in which the analysis indicated both pressure and temperature, while there were 43 perceptions of solidity and 37 cases in which only pressure was reported in the analysis. There were, then, 9 exceptions to the rule that liquidity is composed of a blend of pressure and temperature and that solidity is composed of pressure alone. These 9 exceptions were cases in which a temperature sensation was present, but the meaning was that of solidity because (*a*) the temperature was not blended with the pressure, but surrounded it, or (*b*) the temperature was blended with the pressure and the meaning was that of liquidity until the finger moved and gave a perception of graininess, when the perception changed to one of solidity.

[4] In the 45 experiments in which water was used as stimulus the reports always indicated a blend of pressure and temperature in the analysis of the perception. This verifies Bentley's analysis of liquidity.

The following reports of the *Os* may serve to make clearer this distinction between the conditions underlying the two perceptions.

Ba (meal) "Clear pressure and no temperature. The pressure is in a pattern—a circle of limited extent. It feels like a piece of stretched cloth and is dry."

Ne (meal) "Slight coolness and intense pressure; wet."

S (sand) "There are uneven bits of intense pressure and a slight warmth. It seems soft and doughy. Now the bits of pressure slide, giving a perception of graininess, and it feels like sand."

W (cornstarch) "Pressure which is not very heavy, and neutral temperature. It feels dry and powdery."

Y (warm cotton) "That is warm mercury. There is weak pressure and a slight warmth; not much temperature and the pressure is vague and ill-defined."

S (sand) "Pressure and cold. It felt at first like a very thick gelatine, but suddenly all temperature disappeared and it then meant sand."

Y (flour) "There is a pattern of pressure which is not uniform, and the temperature is indifferent. It is sand or coarse powder. The pattern of pressure gave me the perception of solidity, for the pressure of solids is spread out in a way quite different from that of liquids."

Common sense, as represented by the dictionary, says that a substance is perceived as a liquid when it "flows freely like water," and is perceived as a solid when it "resists forces tending to deform." According to these definitions, liquidity and solidity depend upon the temporal course of experience. According to our reports, however, the perceptions of liquidity and solidity do not depend upon temporal course; for just as good perceptions were obtained when drops of liquid or solid substance were applied to the back of the hand. In these reports, 'solidity' means merely the absence of liquidity.

The first part of our problem, then, may be answered thus: a perception of liquidity is experienced whenever there is a fusion of pressure and temperature, and the perception may be changed to one of solidity by breaking up the fusion, leaving a pattern of pressure alone.

Experiment II.: Perceptions of Liquidity and Semi-liquidity Compared

The second part of our problem was to study a number of liquids of different densities in order to discover the conditions underlying the perception of semi-liquidity, and to compare these conditions with those underlying the perception of liquidity. The stimuli were liquids of various thicknesses, including water, gelatine, bandoline, cornstarch pudding, cornmeal mush, molasses, and mercury. The procedure was similar to that of Experiment I. There were 6 *Os* in this experiment, most of whom had observed in the first experiment.[5]

[5]Miss Olga Nilson, a senior and major student in psychology, assisted us in this series of experiments.

Table II. gives the results of Experiment II. It is arranged to show the number of times intense pressure *plus* temperature was found in the analysis, the number of times weak pressure *plus* temperature was found, and the number of perceptions of liquidity and semi-liquidity reported, first when thick liquids were used as stimuli and secondly when thin liquids were used. The second column of the Table indicates the number of experiments; the third column, the number of times the analysis revealed intense pressure and temperature; the fourth column, the number of times weak pressure and temperature were reported; the fifth column, the number of times the meaning was that of a thick liquid; and the last column, the number of times the perception was of a thin liquid. If the analyses of the *O*s agreed completely, the numbers in columns 3 and 5 and the numbers in columns 4 and 6 would be equal.

TABLE II

Comparison of Perceptions of Liquidity and Semi-liquidity Semi-liquids

Obs.	No. of Exps.	Intense Pres. *plus* Temp.	Weak Pres. *plus* Temp.	Percep. of Semi-liq.	Percep. Liq.
B	1	1	0	1	0
L	19	14	5	17	2
S	20	15	5	15	5
N	3	3	0	3	0
W	23	18	5	22	1
Y	5	2	3	2	3
Total	71	53	18	60	11
			Liquids		
B	2	2	0	2	0
L	2	0	2	0	2
N	2	0	2	0	2
S	6	0	6	0	6
W	4	1	3	2	2
Y	3	0	3	0	3
Total	19	3	16	4	15

The first part of the Table shows 60 cases in which semi-liquids were perceived as thick and 11 cases in which they were perceived as thin, while there were 53 cases in which the perceptions were analysed into a blend of intense pressure and temperature, and 18 cases which showed a blend of weak pressure and temperature. There were, then, 7 cases out of 71 in which the analyses did not agree. Apart from these, semi-liquidity was always analysed into a blend of intense pressure and temperature while liquidity was analysed into a blend of weak pressure and temperature. The second part of the Table, in which liquids were used as stimuli, gives further evidence that weak pressure and temperature when fused mean liquidity. The

4 cases in which a thin liquid meant semi-liquidity instead of liquidity will be explained in Experiment III.[6]

There were 5 experiments which could not be included in the Table because the meaning was different from that of any of the other experiences. Here the *O*s perceived the liquid not as liquid or semi-liquid but as merely damp or vaporous. Ba reported 3 such perceptions, the meaning being at first that of liquidity, and being analysed into cold and pressure, and later changing into a perception of dampness, when the pressure had adapted out. S reported 2 such experiences. Bershansky, in her study of Thunberg's illusion, found perceptions of dampness, which she analysed into a less intense pressure and perhaps a less intense cold than that of wetness.[7] Dampness, then, is the experience which actually contrasts with solidity.

Samples of the reports with different stimuli follow.

B (mercury) "Cold and intense pressure at tip; increase in size and intensity of the pressure and cold; visual imagery giving a panorama of liquid mounting, with fairly definite identification of the liquid as mercury."

L (cornmeal mush) "Warmth, which is intense at first and then decreases in intensity; rather intense pressure, which is greatest at tip of finger. It is thick and spongy."

N (water) "There is a cold coming up the finger in parallel lines and it settles around the finger; there is very little pressure connected with this. It is thin like water."

S (gelatine) "Pressure and temperature, both moderately intense. It means a thick liquid like gelatine."

S (water) "There is cold and a pressure so thin and lacy that it is hardly perceptible. It is merely damp instead of wet. When I have the meaning of dampness I am not sure there is pressure there at all."

W (cornstarch pudding) "Cold although there is no definite cold ring around the top as there usually is. The cold is fused with a close-fitting, clingy pressure which makes it feel thick and wet."

Y (bandoline) "Very cold with a chunk of pressure. It is thicker than water. The meaning of thickness is given by the greater pressure and by the fact that the pressure is more definitely formed than it is in thin liquids."

In order to make sure of our analysis of semi-liquidity, we tried synthetic semi-liquids, using as stimuli substances which were dry and heavy enough to give a rather intense pressure, such as wads of cotton, sometimes warmed or cooled or slightly moistened, and a rubber balloon, partially filled with air, and also warmed or cooled. Out of 12 experiments with cotton, warmed or cooled or moistened, there were 12 reports of semi-liquidity, the meanings being jelly-like, soggy, spongy, muddy, gummy, and doughy. Out of 27 experiments with the balloon, there were 23 perceptions of semi-liquidity, the characterizations being thick syrup, gelatine, mercury, and dough. These were analysed into a blend of moderate pressure and temperature when the meaning was that of a moderately thick liquid, and

[6]Cf. p. 536.

[7]Bershansky, this JOURNAL, 1923, 34, 293.

into a blend of intense pressure and temperature when the meaning was that of a thick liquid or semi-solid. One interesting case where the balloon was used at room temperature, and where the analysis indicated only pressure, resulted in a perception of pumice stone, showing that when the *O*s had been experiencing fusions of pressure and temperature that meant liquidity or semi-liquidity the absence of temperature made them perceive the stimulus as the dryest thing possible. A few of the *O*s' reports for cotton and the rubber balloon follow.

L (balloon) "Pressure and cold. It is a thick jelly-like mass, like mercury, although it does not cling quite as much as mercury."

S (warmed cotton) "Weak warmth and a moderately intense pressure, which clings to the finger. It feels like warm thick cornstarch pudding."

Y (cooled cotton) "A soft pressure, not lumpy and not nearly so well defined as some of the others. A slight cool. It seems somewhat like mud, slightly cool mud. There is a consistency thing about all these; they have differences in consistency and solidity."

Semi-liquidity, as well as liquidity, is conditioned by a fusion of pressure and temperature. The *fusion* of the sensory components is the condition which gives to all liquids, both thick and thin, the meaning of liquidity. The consistency of the liquid resolves itself, upon analysis, into the intensity of the pressure in the fusion; the more intense the pressure, the denser the liquid perceived. The comparison of perceptions of liquidity and solidity in Experiment I. showed a distinct break between the two experiences; there is no such break between experiences of liquidity and semi-liquidity. The two perceptions merge into each other and both of them carry the meaning of liquidity, for both are conditioned upon a fusion of pressure and temperature.

Common sense says that a substance is perceived as a liquid when it falls away from the finger which enters it, and a substance is perceived as semi-liquid when it offers resistance to the entering finger. To be sure, the *O*s often reported a temporal course in their experiences of liquidity and semi-liquidity, but this was not essential to the two perceptions, and was similar for the two. There is nothing on the side of process to distinguish the two perceptions save varying intensities of pressure.

Experiment III.: Semi-liquidity as Conditioned by Increased Intensity of Pressure with Increased Temperature

It may be recalled that Table II. recorded 4 cases in which water was perceived as semi-liquid.[8] These 4 cases occurred when the water used was colder than usual. This result with cold water brought to mind the results obtained with warm water in our work on oiliness.[9] There we found that increase in

[8] Cf. p. 535.

[9] Cobbey and Sullivan, this JOURNAL, 1922, 33, 121.

the warmth of the water brought about an increase in the snugness of the pressure, and we were inclined to consider this phenomenon to be of central origin, the pressure *feeling* snug simply because it was blended with warmth. Bershansky, in repeating our experiments, found the same phenomenon, and says: "It is of course possible that the pressure in this case has its origin internally, and is referred to the surface. Internal pressure is, however, frequently reported, and we should still have to explain why increase in warmth of stimulus carries with it increase in felt pressure."[10] Since Experiment II. of this study had given us a hint that increase in cold also carries with it increase in felt pressure, we decided now to run through a series of experiments, using water of all degrees of temperature from 0° to 40°C. If increase in cold or warmth resulted in increase in intensity of pressure, water of different degrees of temperature should give perceptions of semi-liquids of varying degrees of thickness. Since there were already a number of cases reported in which increase in warmth brought about increase in intensity of pressure, especial emphasis was laid upon the experiments with cold water. The results of this series of experiments are given in Table III.

TABLE III.

Perception of Semi-liquidity from Cold and Warm Water: Water 0° to 15°C.

Obs.	No. of Exps.	Percep. of Semi-liquidity	Meanings
B	2	2	Mercury; heavy liquid
Ba	4	2	Denser than water, less than mercury
Bl	3	2	Mercury
L	8	5	Mercury, melting snow
Ne	6	6	Gelatine; thicker than mercury
N	5	3	Gelatine; kerosene
S	9	7	Melting snow; between water and mercury; mercury
W	5	1	Molasses
Y[11]	7	2	Between water and mercury
Total	47	30	
		Water 38° to 40°C.	
L	13	13	Oil; butter
S	5	5	Oil; melted butter
W	4	3	Mercury; oil
Y	3	2	Thicker than water; oil (?)
Total	25	23	

Total for warm and cold water: of 72 experiments, 53 perceptions of semi-liquidity.

[10] I. Bershansky, this JOURNAL, 1922, 33, 584.

[11] After Y had reported two experiences of semi-liquidity with cold water as stimulus, he was told of the conditions of the experiments, and thereafter he always perceived the stimulus as ice-water. This indicates the importance of keeping *O* in complete ignorance of the conditions of the experiment, especially when *E* is trying synthetic conditions.

The results shown in Table III. bear out the findings of the previous investigations in regard to an increase in pressure with an increase in warmth, and add the fact that there is an even more pronounced increase in intensity of pressure with an increase in cold. Every one of the 9 *O*s perceived very cold water as semi-liquid at times, and every one of the 4 *O*s who observed in the experiments with warm water perceived this as semi-liquidity. Out of 47 experiments with ice-water, there were 30 reports of semi-liquidity, the perceptions varying in density with the intensity of the cold, and running from liquids perceived as slightly thicker than water to liquids perceived as thick as partially melted snow. When warm water was used, there were 23 perceptions of semi-liquidity out of 25 experiments, the meanings here being most often oiliness or greasiness. There were, then, 53 cases out of 72 experiments in which an increase in the temperature of the stimulus resulted in an increase in the intensity of felt pressure, making the perception that of semi-liquidity.[12] Some examples of the *O*s' reports follow.

B (water 10°C.) "Cold of considerable intensity, with fairly strong pressure, which is well defined at end of finger. Variable intensity and extent of both pressure and cold. First there was visual imagery which meant 'There is a liquid stuff coming up the end of my finger.' It seemed to mean something there a little heavier than water. There were also verbal comments in auditory kinaesthesis, which meant 'Ah, there is something cold and wet, possibly mercury; no, it is more like water.' "

L (water 15°C.) "Intense pressure and very cold, which rise in parallel lines. It is slightly thicker than water."

S (water 8°C.) "Intense pressure and subcutaneous pressure plus cold, which is quite intense. The cold is not as clear as the pressure. I could feel the finger cut through the liquid which clung to the finger more than water does. It is very thick like partially melted snow."

Y (water 8°C.) "Intense cold and a ring of pressure going up the finger. It is fairly thick and there is some resistance. It meant a liquid that at first I thought might be water, then mercury. It really was between the two."

These reports show that very cold water is perceived as a semi-liquid. Under the conditions of this experiment only water of a very limited range of temperature is perceived as water or liquid, while water of extreme temperature is perceived as gelatine or mercury or even partially melted snow. The initial contact of the finger with the liquid is the time when thickness is best perceived with cold water. It is at this point

[12]It is interesting to speculate as to the cause of this change in intensity of pressure with change in temperature. We are now engaged in experiments in an attempt to find the explanation. At present we are inclined to think that the vascular change, which is known to occur when the skin is subjected to extremes of temperature, stimulates, in some way, the subcutaneous pressure organs. Another explanation, suggested by Dr. Young, is that a cold or warm stimulus may affect the pressure organs in the skin, as well as the temperature organs.

that most of the reports of extreme thickness, such as mercury or partially melted snow, were given. The skin very quickly adapts to the cold and then the intense pressure immediately decreases. Then the perception undergoes a change.

When warm water was used as stimulus, the perception was that of a liquid slightly thicker than water which meant, in most cases, oiliness. Out of 25 experiments with warm water there were 18 reports of oiliness. Since oily liquids were not used as stimuli, the *O*s could not have perceived oiliness because of suggestion. The following reports of the *O*s are typical.

L (water 40°C.) "Warm, medium pressure, thicker than water. Does not cling to finger. Feels oily."

S (water 38°C.) "It feels like melted butter. The pressure is fairly weak and is blended with an intense warmth. It is a little thicker than water."

Y (water 38°C.) "Warm pressure, wet; I started to say oily; there was just a suggestion of oiliness. The perception was of a warm liquid thicker than water."

It is evident that the perception of oiliness includes the meaning of thickness. This fact may account, in part at least, for Bershansky's disagreement with our earlier study of oiliness, where we said that the compulsory condition for the perception of oiliness was a fusion of warmth and pressure.[13] Bershansky says that oiliness is not a "sheerly cutaneous perception", and this is true. She also says that a fusion of warmth and pressure is not "the single adequate condition" for the perception; but we still fail to find anything else on the side of process. In Experiment III., cold water was perceived as wet, and when the water was heated to a certain temperature it was perceived as oily. It is admitted that wetness is conditioned by a fusion of cold and pressure; and if wetness is changed to oiliness simply by changing the cold to warmth, it is reasonable to say that oiliness is conditioned by a fusion of warmth and pressure. It may be that the internal change, whatever it may be, which causes an increase in the intensity of pressure with an increase in temperature, does not occur except in cases where the stimulation is areal. It would be better, therefore, to lay down the conditions for the perception of oiliness, as well as for all other perceptions of semi-liquidity, for areal stimulation only. Later, with further experimentation, we hope to be able to say definitely what the results of punctiform stimulation are.

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[13] I. Bershansky, this JOURNAL, 1922, 33, 584. See also Cobbey and Sullivan, this JOURNAL, 1922, 33, 121.

By changing the temperature of water, it is possible to get perceptions which change in density so that the whole series of liquid, semi-liquid, solid integrations is represented, thus:

0°C—mushy (partially melted snow)
10°C—muddy (mercury)
15°C—gelatinous (gelatine)
25°C—wet (water)
38°C—oily (oil)
42°C—greasy (melted butter)

It is too soon to say, of course, that the compulsory condition for the perception of mushiness, which may carry the object-meaning of partially melted snow, is water of 0°C., or that one is certain to perceive water at 15°C. as gelatinous, for individual differences are quite pronounced; but it is possible to say that the perceptions most often obtained with water of certain temperatures are the perceptions given in the column above.

This study of liquids and semi-liquids and synthetic semi-liquids makes it possible to enumerate the experiences included in the term liquidity. We found a series of perceptions beginning with dampness or vaporousness, running through all experiences of liquidity and semi-liquidity, and ending with the experience which contrasts with vaporousness, namely, solidity. Thus liquidity is bounded on the one side by vaporousness and on the other side by solidity. Such a series includes the following experiences:

Liquid	Semi-liquid	Semi-solid	Solid
Vaporus, wet, oily,	gelatinous, slimy, greasy,	muddy, mushy, soggy,	doughy soggy, dry

If we let 'p' represent weak pressure, 'P' represent moderate pressure, and 'P' represent strong pressure, 'c' represent weak cold, 'c,' moderate cold, and 'C,' strong cold: 'w' represent weak warmth, 'w,' moderate warmth, and 'W,' strong warmth; the foregoing perceptions may be pictured in terms of their sensory components thus:

Vaporous,	wet,	oily,	gelatinous,	greasy,	mushy,	doughy,	dry
C(p),	Cp,	WP,	cP,	wP,	cP,	wP,	P(w)

Conclusions

1. All perceptions of liquidity have in common one condition, namely, a fusion of pressure and temperature. This fusion is lacking in perceptions of solidity, in which there is, in most cases, only a pattern of pressure. The relationship between liquidity and solidity may be shown thus:

Liquidity	$\propto$	Solidity
pT	$\propto$	P(t)

2. All perceptions of semi-liquidity, as well as those of liquidity, are conditioned by a fusion of pressure and temperature.

It is impossible to say just where liquidity ends and semi-liquidity begins, the only difference being one of intensity of the pressure. In most cases of semi-liquidity the intensity of pressure and temperature is about equal. The more intense the pressure, the denser is the liquid perceived. The relationship between liquidity and semi-liquidity may be represented thus:

Liquidity	$\propto$	Semi-liquidity
pT	$\propto$	PT

3. Liquidity is a broader term than wetness or oiliness or semi-liquidity; it includes all these and a great many more perceptions. It is possible to arrange a series of perceptions beginning with experiences of liquids so thin that they are merely damp or vaporous and running through all the intermediate stages to solidity. Such a series includes the following perceptions:

Vaporous, wet, oily, gelatinous, slimy, greasy, muddy, mushy, soggy, doughy, spongy, dry.

The relationship between the end terms and the middle terms of these experiences may be shown thus:

Liquidity	$\propto$	Semi-liquidity	$\propto$	Solidity
(p)T	$\propto$	PT	$\propto$	P(t)

4. There is a decided increase in the intensity of pressure with an increase in the intensity of the cold or warmth in the fusion which conditions perceptions of semi-liquidity. This enables one to obtain good synthetic semi-liquids by using cold and warm water as stimuli.

5. The large class of experiences included in the term 'liquidity' are true blends or fusions, but the equally large class of experiences included in the term 'solidity', such as dry, soft, hard, etc., are really not touch blends at all, if we mean by touch blends fusions or integrations. They are, rather, cutaneous patterns or mosaics.

References to Studies on Touch Blends

1. M. Bentley, this JOURNAL, 1900, 9, 414.
2. I. Bershansky, this JOURNAL, 1922, 33, 584.
3. I. Bershansky, this JOURNAL, 1923, 34, 291.
4. L. W. Cobbey and A. H. Sullivan, this JOURNAL, 1922, 33, 121.
5. R. S. Malmud, this JOURNAL, 1921, 32, 571.
6. S. Tung, this JOURNAL, 1921, 32, 421.
7. M. J. Zigler, this JOURNAL, 1923, 34, 73.
8. M. Meenes and M. J. Zigler, this JOURNAL, 1923, 34, 542.
9. M. J. Zigler, this JOURNAL, 1923, 34, 550.

AN EXPERIMENTAL STUDY OF THE PERCEPTIONS ROUGHNESS AND SMOOTHNESS

By M. Meenes and M. J. Zigler, Princeton University

In 1903 Thunberg[1] reported that a singular perception of smoothness is touched off by holding the hands, one on each side of a vertical network of metal, in such a way that the volar surfaces of the hands and fingers touch each other through the network openings, and drawing them proximally so that they slide over the wire network always in the same mutual relation. The same perception is realized under identical conditions when the metal network is replaced by other obviously non-smooth objects, such as sail-maker's cord. Thunberg concludes that this perception is a complex experience, which results from a fusion of two different perceptions, each derived from separate portions of the skin that are in immediate proximity during the stimulation. He says that the experience fails to arise when only one hand is moved over the network and when the volar surfaces of the hands are moved back and forth against each other. This perception of smoothness, he says, analyses into a uniform perception of touch and a perception of facile motion of the sensitive surface in relation to the touched object. He emphasizes the fact that only a poor perception of "the degree of smoothness that characterizes an object" is obtained by merely touching the object, even with such a very sensitive part of the skin as the finger tips; hence, movement is indispensable to the perception.

In view of the fact that this perception of smoothness was aroused under unusual and complex conditions, and that Thunberg speaks at times of smoothness as characterizing the stimulus-object, we felt that it might prove fruitful psychologically (1) to repeat certain parts of his work in order to secure, if possible, a fuller description of the psychological experience of smoothness, and to ascertain whether movement is an indispensable condition of the perception, and in that event to determine the precise rôle of this factor in the perception; (2) to simplify the conditions of his experimental work by confining the stimulation to one portion of skin instead of stimulating two juxtaposed surfaces; and (3) to extend his study so as to include also the counterpart experience of roughness, and to ascertain whether these two perceptions represent the extremes of a simple qualitative psychological series between which a number of corresponding intermediate qualities fall.

[1] T. Thunberg, An egenartad Förnimmelse of Glatthet och des Analys, *Upsala Läkareförening, Förhandlingar N.F.* 8, 1902–3, 660–663.

Analysis of Smoothness and Roughness

(1) As our first experimental step we attempted to determine whether stimuli which are popularly termed rough and smooth give rise to such perceptions when presented without movement. The stimuli selected for this purpose were a series of 12 papers of varying degrees of roughness and smoothness; a smooth glazed paper represented one extreme and a coarse sandpaper the other, and 10 other papers of varying degrees of roughness and smoothness occupied the intermediate positions. For convenience the stimuli were numbered 1 to 12, from most smooth to most rough. These were presented in haphazard order by means of an apparatus described by Zigler,[2] which was altered, however, by removing the spring and metal disc applicator and substituting in its place a long iron rod over the lower end of which the papers serving as stimuli were clamped. With this apparatus we were able to bring the stimulus down upon the volar surface of the tip of *O*'s forefinger, hold it there steadily for a period of 3 sec., and then remove it directly upward without any movement excepting those in the vertical plane at the moments of application and withdrawal.

There were four *O*s: Mr. Norborne H. Crowell (C), graduate student; Miss Janice H. Ozias (Oz), editorial assistant of the Psychological Review; Mr. Frank A. Pattie (P), graduate fellow; and the last named of the joint authors (Z), who worked with knowledge of the problem. The *O*s were blindfolded and seated before a screen with an aperture through which they inserted the right hand, volar side upward. The instructions read: "Two seconds after the signal 'Now,' a stimulus will be applied to the forefinger of your right hand. Observe carefully the field of consciousness during the stimulation and at its termination give a detailed account of the experience."

In this series of experiments only Oz designated the experiences as smooth and rough, and she did not give positive declarations of these qualities but inclined to use such qualifying remarks as; "almost smooth," "gritty or almost rough," "rough but can hardly be called so," "that would hardly be called rough." She reported further that these impressions of smoothness and roughness were perceived mainly at the moments of application and withdrawal of the stimulus, when there was, of course, vertical movement. Thus for stimulus 11 she says; "A little gritty and rough at first, then unevenness of pressure sensations for the main part of the period."

None of the other *O*s named these experiences smooth or rough. They did not use these terms, excepting in an occasional negative connection, when an experience was designated as not rough or not smooth. These *O*s designated the experiences occasioned by this mode of stimulation as evenness and uneven-

[2] An Experimental Study of the Perception of Stickiness, this Journal, 1923, 34, 75f.

ness. Oz also inclined to designate the major part of the experiences by the same labels. For stimulus 12 (grade 3 sandpaper) she reported: "Surface uneven, an unevenness of pressure sensations. There were greater pressure sensations in ridges with slighter pressure sensations between them." And for stimulus 2 she said: "An even surface giving rise to evenness of pressure sensations over the entire area."

It seems, then, that when differing intensities of pressure sensations are experienced in the absence of movement, our *O*s label the perception unevenness and not roughness; and when the pressure pattern is uniform while lacking movement, the preferred name for the perception is evenness and not smoothness.

The *O*s described evenness as consisting of a field of pressure qualities in which all pressure elements have the same level of intensity and clearness and a closely aggregated distribution, but lack the compactness of aggregation, which is characteristic of smoothness. Unevenness consists of an areal pressure pattern in which certain small aggregations of pressure possess a higher intensity and clearness than others, and in which these aggregations of higher and lower intensities maintain practically the same spatial positions throughout the experience.

(2) Failing to secure reports of genuine roughness and smoothness when both stimulus and stimulated member were passive, we next added activity of the member to the experience. *O* was now instructed: "After the 'ready' signal you will be presented with a stimulus which will be placed on your passively outstretched forefinger. Then, when the 'now' signal is given, you are to drop the thumb lightly on the stimulus directly over the forefinger without the slightest rubbing of the fingers over the stimulus. At the second 'now' signal, carefully lift the thumb from the stimulus, which will then be removed by *E*. Try to name the tactual experience and give a careful introspective account of it." The same series of stimuli was used.

Here occasional judgments of roughness and smoothness appear in the reports of all *O*s. Z, however, reports no clear-cut perceptions of smoothness and roughness, although he finds suggestions of them. All *O*s again prefer to label these experiences evenness and unevenness of pressure. They indicate that the suggestions of smoothness and roughness arise during the initial contact of the thumb with the stimulus when slight movement is involved. The following reports are typical:

"That experience was evenness of pressure, but during the movement of the thumb in clamping the stimulus I felt slight roughness" (P). "A brief gritty or rough perception was set up during the clamping stage, but this was immediately succeeded by mere unevenness of pressure, which endured until at the release of the stimulus I again got a suggestion of roughness" (Z). "At first roughness, then I just felt unevenness" (C).

The reports of two *O*s indicate that the finger is very obtuse to slight differences of pressure when both stimulus and stimulated member are stationary, while sensitivity is much heightened with the introduction of movement.

"Pressure sensations of different intensities which give rise to unevenness; and it even felt rough and gritty in very definite parts; but when the pressure of the thumb continues to be applied these differences of pressure disappear and blend into one sensation which I call evenness. When the thumb is raised again at the end of the experience I am again aware of the differences of pressure" (Oz). "Unevenness was perceived as soon as I dropped my finger upon that stimulus. There were several tiny points of increased intensity of pressure which gave this impression. This appeared immediately upon the dropping of my thumb, and then until I elevated my thumb I was totally unaware of this unevenness; the experience was evenness during this period. When I raised my thumb, however, I again received the suggestion of slight unevenness" (Z).

(3) In the next experiment the stimulated member was kept passive, all movement being confined to the stimulus.

A special apparatus was devised to carry the moving stimulus. It consisted of a phonograph motor encased in a mahogany box 30 cm. square and 15 cm. high. The vertical shaft, which supports the phonographic disc platform, extended through a small hole at the mid-point of the mahogany box about 15 mm., and a small cogwheel was substituted in place of the platform. This wheel was geared with a larger toothed wheel, which bore a metal radius-block on its upper surface. The radius-block was pierced by 5 holes at regular intervals from center to circumference of the cogwheel, and one end of a straight horizontal shaft 9 cm. in length was fastened to one of these holes by a screw. The radius-block was of sufficient thickness to allow the shaft to clear the center-tap of the cogwheel during rotation. The other end of this shaft was pivoted with the end of a longer straight horizontal shaft 17.5 cm. in length, which ran off at an angle of about 90° from the former. This longer horizontal shaft was centered on a post, which was located at the edge of the mahogany casing; thus, one-half of this shaft extended over the edge of the box. The stimulus carrier, which was controlled by guides at the edge of the box so that it moved along the side of the box without making any lateral movement, was attached to the outer end of this long shaft by means of its center-pin, which played in a slot in the outer end of the shaft. Thus, as the cogwheels rotated under the driving force of the motor, the carrier made a reciprocating movement, carrying the stimulus back and forth.

A delicately balanced horizontal arm was suspended from the center-pin of the carrier. On one end of this arm a cylindrical terminal, 2 cm. in diam., was fitted and the stimulus was clamped over the end of this terminal. A counter-weight, placed on the opposite end of the arm at a like distance from the center-pin, balanced the terminal. The arm was graduated in units of 10 gr. in both directions from this zero point, so that the pressure of the stimulus could be applied to the finger either from below or from above. The extent of the movement varied from a maximal complete excursion of 15 cm. to a minimal of 4.8 cm. with three intermediate distances. Since we stimulated the fingertip only, the shortest distance was most appropriate for our work. Although this apparatus was not constructed to insure absolute regularity of movement, it achieved this end satisfactorily for our work. In all of our work the stimulus carrier moved at the rate of one full excursion in 3.43 sec.

Every *O*'s hand was provided with a plaster cast which allowed only a little more than the first joint of the index finger to be exposed. The hand rested in the cast dorsal side upward. This enabled us to keep *O*'s hand

perfectly stationary and also to eliminate the fatigue that is usually occasioned by holding the arm volar side upward for a considerable period of time or by failure in having the weight of the member distributed over a considerable portion of its surface. *O* was again both screened and blindfolded from the stimulated part.

To our original series of stimuli we here added an additional one—a highly polished piece of brass—which better satisfied the demands of extreme smoothness. This became No. 1 of our series of 13 stimuli. At every presentation the stimulus carrier was first set in motion, then the stimulus was applied to the finger tip and allowed to make about 3 full excursions before it was removed and the machine stopped. Thus, the stimulus was in motion throughout the period of stimulation. We began with a pressure of 20 gr. and later took observations with pressures of 30, 50 and 70 gr. The instructions read: "After the 'ready' signal a tactual stimulus will be applied to your finger. Observe the experience carefully and at the end of the stimulation name the perception and give a complete introspective account of the experience in terms of the mental processes."

Under these experimental conditions all *O*s gave positive declarations of smoothness and roughness. Other qualities, such as grittiness, scratchiness, and mixtures of smoothness and roughness also appear very frequently, and evenness and unevenness were named in a few instances, but these two qualities seem to be obscured here by the other more insistent qualities first mentioned.

The experience of smoothness is described as consisting of an areal pressure pattern, essentially cutaneous, in which all the pressure elements are temporally sustained at a uniform level of intensity, have a uniform clearness, and are closely compacted spatially. There are no intervening unstimulated areas or points, nor any areas of lower or higher intensity or clearness, in a genuine smoothness. The experience is at its fullest when the pressure pattern has a low or moderate order of intensity. The degree of compactness of pressure bears a direct ratio to the degree of smoothness. Smoothness is introspectively distinguished from the perception of evenness by the extreme compactness of the adjacent pressure elements and by the higher level of clearness which characterizes the total pattern.

The perception of roughness is constituted of an areal pressure pattern of both cutaneous and subcutaneous elements and is characterized by a lack of uniformity of stimulation of the different elements composing the complex. Certain of the pressure points or subareas have a high order of intensity, others are low or moderate, while distributed among these are intervening unstimulated areas. Moreover, the points of maximal intensity remain so only momentarily, and are succeeded by other points or small areas, which wax and wane in intensity within a brief temporal interval and are succeeded in turn by others presenting the same characteristics. Thus, there are constantly increasing and decreasing fluctuations of intenstiy and corresponding shifts of the point of focal clearness among the various elements consti-

tuting the pattern. Z points out that a genuine roughness is not realized unless there are unstimulated points or spots in the pattern. The other *O*s fail to make this distinction, indicating that full-fledged roughness appears when two or more orders of intensity of pressure, which manifest the characteristic changes of intensity and clearness, are represented. Roughness is distinguished from unevenness chiefly by these constant and rapid shifts of intensity and clearness. Like roughness, unevenness is characterized by two or more orders of intensity and clearness, but these remain relatively constant and stable throughout the experience in the latter case.

Movement is reported by Z as being characteristic of the stimulus rather than of the psychological experience. The other *O*s rarely mention movement, and where they do so it is usually in a casual parenthetical remark at the end of the report, to the effect that the stimulus moved to and fro on the finger. The rare reference to movement perhaps points indirectly to the fact that movement is not a psychological datum of the experience; the *O*s were instructed to report in full the psychological characteristics of the experience and should have reported movement at every presentation if it contributed to the psychological experience. Movement is, however, an indispensable condition of the stimulus, since without it neither smoothness nor roughness is realized. In smoothness, movement seems to render more compact the aggregation of pressure elements and to make clearer the uniformly stimulated pressure pattern. In roughness, movement provides the means of stimulating intermittently the different aggregations of pressure which successively undergo sudden changes in intensity and clearness.

Increasing the amount of pressure to 50 and 70 gr. called out no distinctly new qualities, although scratchiness appeared more frequently in these reports. This quality appears to be a type of roughness in which an incipient or actual pain element is added. Smoothness does not appear at its fullest when the pressure of the stimulus is excessive, owing to the presence of a complex of subcutaneous sensations which contribute little if anything to smoothness, and perhaps also to an apparent reduction of cutaneous sensitivity under these conditions. Roughness is not altered so much here, since the "slight pulls" or successive fluctuations of pressure intensities derive in part from subcutaneous organs; at the highest application of pressure, however, there is a certain loss of the configurative detail of the roughness pattern, owing either to a blending of the cutaneous and subcutaneous elements or to a reduction in sensitivity of the cutaneous organs. Grittiness is described as a special kind of roughness, characterized by punctiform points of changing intensity which are separated by very small intervening unstimulated areas.

The *O*s imply through their reports of intermediates that smoothness and roughness represent a single qualitative series. Thus, Oz reports: "Roughness with an element of smoothness"; and to the same stimulus at another time: "smoothness with an element of roughness" (Stimulus 8). For Stimulus 5 she says: "a slightly rough smoothness." Z tended to report certain experiences as half way between roughness and smoothness: "if we regard a straight line as representing the scale of smoothness-roughness series, this experience would stand 40 points from the rough extreme" (Stimulus 8). P reports for Stimulus 6: "that was a smoothness with a slight suggestion of roughness." All *O*s inclined more or less to characterize certain experiences as rough smoothness or smooth roughness.

Since smoothness was designated as essentially cutaneous and roughness as largely cutaneous, we next anaesthetized the tip of the forefinger with ether and presented some of the same stimuli with movement. We found it very difficult to rule out cutaneous sensibility completely, and with one *O* in particular we were never quite able to secure this end. In the more successful cases the presentation of smooth stimuli gave rise to a mere dull diffuse pressure, which apparently derived from subcutaneous organs. There were no reports of smoothness at all. Roughness was not completely ruled out by complete anaesthesia, however, although most of the configurative detail of genuine roughness is lost. We are thus led to the conclusion, already indicated by the *O*s, that smoothness is almost entirely cutaneous, while roughness is both cutaneous and subcutaneous.

Synthesis of Smoothness and Roughness

Having secured analytical descriptions of these perceptions, we next endeavored to find stimuli that ordinarily are not designated as smooth and rough which might possibly fulfill the requirements for touching off these perceptions. A small portion of the tip of *O*'s finger was exposed through a small hole in a piece of cardboard which covered his finger, and a jet of air from the laboratory air-pressure system was applied to the exposed part of the finger. *O* was blindfolded, his ears stopped with cotton, and a noisy electric motor was set going to prevent his detection by visual or auditory cues that the air system was operating. All *O*s reported smoothness here. Sometimes the smoothness was fused with other qualities, such as softness and coolness, but all described typical experiences of smoothness.

For roughness we used very, very weak faradisation by terminals which, when moving over the skin, gave rise to distinct smoothness. The terminals were either moved over *O*'s finger or *O* was instructed to move his finger over the terminal. Here all *O*s except P reported roughness, Oz and Z at nearly every stimulation. They indicated, however, that the experience was blurred by the sensations of tingle, prickle and tickle. Although P failed to mention roughness, he described pressure sensations having different and changing degrees of intensity.

Professor H. C. McComas also observed in this part of our work and reported, along with other tactual experiences, several instances of slight roughness.

Conclusion

Smoothness is a simple tactual perception consisting of a field of cutaneous pressure qualities, all of which possess a low or moderate and uniform intensity, and a relatively uniform clearness, and are so closely aggregated and blended as to give rise to an impression of extreme compactness.

Roughness is a tactual perception constituted of an areal pattern including both cutaneous and subcutaneous pressure elements. This perception is characterized by a lack of uniformity of stimulation. In extreme roughness the pattern lacks spatial compactness, in that intervening unstimulated areas are scattered among the stimulated pressure points or sub-areas. The scattered stimulated parts of the pattern are characterized by several levels of intensity, and are constantly undergoing intensive change, while the points of focal clearness are also constantly shifting among the stimulated parts.

Neither smoothness nor roughness is realized when movement is lacking. In the absence of movement the experiences are labelled evenness and unevenness. Evenness differs from smoothness psychologically by the lack of a high degree of clearness and by the lack of extreme compactness. Unevenness differs from roughness in that the parts of higher intensity of pressure retain their relative prominence throughout the experience, hence it is characterized by stability of stimulation.

Movement is indicated as being a characteristic of the stimulus, and not a psychological datum of the experience. In smoothness, it serves to clarify the pressure elements and to fuse them into an extreme compactness, while in roughness it secures intermittent stimulation of the parts which successively undergo changes of intensity and clearness in brief temporal compass.

AN EXPERIMENTAL STUDY OF THE PERCEPTION OF CLAMMINESS

By M. J. Zigler, Princeton University

In setting about the task of analysing and determining the compulsory conditions of the perception of clamminess, we first endeavored to find a stimulus which would give rise to a clammy experience, if possible, as often as it was presented to an *O*. So far as our knowledge goes, the only references to clamminess which appear in the experimental literature are those of Titchener, who in one instance[1] says this perception resolves into cold and soft, where the cold sensations and the pressure elements in the softness are so distributed as to occasion the perception of moisture, and on another occasion[2] testifies that a "horribly clammy feel" is touched off when the finger is set upon a bit of cold boiled potato in the dark. On this authority, we selected as the stimulus for our initial experimental work that mentioned in the latter reference.

(1) The fresh cut surface of a bit of boiled potato was applied to the tip of *O*'s index finger by an apparatus (slightly modified) previously described in connection with another experimental study.[3] The boiled potato was cooled to a temperature considerably below physiological zero, roughly between 15 and 25° C., and cut into small cylindrical pieces of a uniform diameter of 12 mm. One of these cylindrical pieces of potato was pressed into a small inverted metal cup, also of 12 mm. diameter, which was substituted for the metal disc applicator of the old apparatus. This apparatus enabled us to bring the stimulus vertically downward upon *O*'s finger tip, where it was held steadily for 5 sec. and then raised again. *O*'s finger was passive throughout, and there was no movement of the stimulus excepting in the vertical direction at the moments of application and of removal. *O* was blindfolded on entering the room and was also screened from the hand which was stimulated.

The *O*s were Mr. N. H. Crowell (C), graduate student; Mr. M. Meenes (M), assistant in psychology; Miss Janice H. Ozias (Oz), a Smith College graduate, now editorial assistant for the Psychological Review; and Mr. Frank A. Pattie (P), graduate fellow in psychology. Mr. Paul C. Squires (S), graduate student, also observed in Experiments 5 and 6. All *O*s were untrained in tactual observation; but after a brief preliminary training period they all began to use the accepted terminology, and rendered fairly full and accurate descriptions. We then took a total of 50 observations

[1] E. B. Titchener, *Textbook of Psychology*, 172.

[2] E. B. Titchener, *A Beginner's Psychology*, 47.

[3] M. J. Zigler, An Experimental Study of the Perception of Stickiness, this Journal, 1923, 34, 76.

from every *O* by the method of procedure described above; 10 observations constituted a series and only one series was presented at a sitting. The *O*s were instructed: "After the 'ready' signal a stimulus which will set up a tactual perception will be applied to the volar surface of the tip of your forefinger. Observe this perception carefully, and at the end of the experience, name it, if possible, and describe in full all of its sensory characteristics."

All *O*s stated that no single tactual term was comprehensive enough to cover all of the sensory qualities occasioned during the stimulation. We obtained abundant and consistent reports of cold, soft, moist or damp, and quite a number of reports of wetness, stickiness and adhesiveness. But we received very few references to clamminess, a total of 10, all of which were rendered by a single *O*, M. Six of these appeared in the second series, and the other 4 in the third; Series 1, 4 and 5 received not a single mention of clamminess.

(2) Failing by this method of attack, we next introduced movement into the perception in the hope that this condition, so indispensable to some of the touch blends, might supply the necessary basis for arousing clamminess. The same stimulus, a cylindrical piece of cold boiled potato of 12 mm. diam., was carried in the cup of the moving arm of a piece of apparatus described elsewhere.[4] The stimulus made a reciprocating movement, a total excursion of 4.8 cm., and pressed against *O*'s finger with a total pressure of 20 gr. The same instructions were used as in Experiment 1, and 5 similar series of 10 observations per series were taken from every *O*.

The reports of this bit of experimentation bear a number of new tactual qualities, most of them obviously attributable to the introduction of movement; *e.g.*, smoothness, grittiness, greasiness, oiliness, slipperiness, granulation of surface; but the references to clamminess were even rarer than in the initial experiment. Only one *O*, again M, used the term, this time only twice in a total of 50 presentations, and both of these allusions appeared in the same series.

The dozen references by this one *O* in the work so far were not distinctly outspoken and positive declarations of clamminess. In nearly every instance he made remarks such as: "There was a suggestion of clamminess," or "It was perhaps a little clammy;" and several times the allusion was negatively stated, as: "It was not much clammy," or "It was hardly clammy." He did not give a positive statement or implication that any perception was a distinctive and unqualified clamminess. Moreover, the use of the term usually appeared at the end of the report after most of the other characteristics of the experience had been reported.

[4] M. Meenes and M. J. Zigler, An Experimental Study of the Perceptions Roughness and Smoothness, this JOURNAL, 1923, 34, 542.

(3) For this one *O*, at least, movement of stimulus seemed to make the realization of clamminess less likely than presentation without motion; consequently, movement was eliminated in the next experiment. We now varied the nature of the stimulus, hoping to find a more effective one. We tried in order plasteline, raw clams, oysters, bread-dough, putty, clay, moist absorbent cotton, raw potato and fresh ground meat. In every case the stimulus was cooled to a temperature of between 15 and 25° C., and was presented for two full series of 10 presentations each. In those cases where the stimulus left any residue or odor, which might apprise *O* of its nature, *E* bathed *O*'s finger with absorbent cotton saturated with gasoline at the end of the observation period. The instructions remained unchanged.

This entire period of experimenting failed to yield 'clammy' reports of any consequence. The only *O* to use the term was M, who employed it only two times, both at a single sitting with plasteline as the stimulus.

(4) There were several hints in the reports of this one *O* that an 'object-attitude' was assumed in certain instances where the perception of clamminess was aroused. In one instance, after he had reported all the sensory characteristics, such as cold, soft, moisture or dampness, he remarked: "That was not unlike the finger of a cold clammy hand. It was a cold, clammy, unpleasant experience." This gave us the suggestion that clamminess might be partially conditioned upon a central or imaginal contribution of *O* to certain kinds of stimulus-objects. Acting on this suggestion, we arranged as stimuli for our next experiment (1) a rubber glove filled with cold water, (2) a kid glove filled with moist sand, (3) a kid glove filled with moistened bread crumbs, and (4) a kid glove filled with moistened oatmeal. These were presented in irregular order, a total of 50 presentations for every *O*, by being dropped across the palm and fingers of *O*'s open hand. The instructions remained the same, excepting a slight change to the effect that the stimulus would be applied to the palm as well as to the fingers.

The results of this experiment proved more encouraging. Here all 4 *O*s reported clamminess; Oz 16 times, M 14 times, P 25 times, and C 6 times. Stimuli 3 and 4 were more effective in touching off the perception than 1 and 2, which elicited 5 and 2 'clammy' reports respectively. Clamminess was analyzed into two constituent parts, the one essentially sensory, the other imaginal. The sensory or peripheral component of clamminess breaks up into cold, a yielding or "givy" softness, and the perception of moistness or dampness.[5] Cold and moistness or dampness were

[5] Cf. Titchener, *op. cit.* Dampness and moistness are themselves touch blends, not simple touch qualities. See M. Bentley, The Synthetic Experiment, this JOURNAL, 9, 1900, 414ff.

indicated as the more obtrusive aspects of the complex, although in the absence of softness, as was practically true in the case of Stimulus 2, we received scarcely any 'clammy' reports. The rubber investment of Stimulus 1 proved inadequate, not so much because it failed to arouse the perception of moisture, as because the *O*s detected that it was rubber; hence the stimulus did not suggest to *O* a distinctly unpleasant stimulus-object, a characteristic seemingly essential to clamminess, as we shall indicate presently. The other 3 stimuli were actually objectively moist on the outside, owing to the wet inner contents of the gloves seeping through.

As a general rule the *O*s reported all of the sensory characteristics of the experience first, without mentioning clamminess; the references to clamminess appeared at the end of the report, when *O* at times obviously shifted from the attitude of describing the sensory data to an attitude of characterizing the stimulus-object or the imaginal and emotional data suggested by it. The *O*s stated that the most markedly clammy experiences were those in which the stimulus suggested a distinctly and unexpectedly unpleasant stimulus-object. Moreover, it was only in those instances where the object-attitude was quite evident that an immediate use of the term appeared in the reports. All *O*s asserted definitely or implied that clamminess is always unpleasant and is imaginally associated with a disagreeable stimulus-object. The following excerpts from the reports indicate these several facts.

"It was a cold clammy thing. I was much surprised nd slightly shocked. It felt like the fingers of a hand. I had strong visual associations of dead man's hand, a game played at Hallowe'en parties" (Oz). "That felt like a 5-fingered hand thrown across my own hand. It was a distinctly clammy hand" (M). "Clamminess as of a putty hand" (M). "It felt like a glove. I felt the cold clammy fingers. It was distinctly disagreeable" (P). "That was clammy. It called up a visual image of a crawling snake" (C). "That was clammy like filthy rags" (C).

The unpleasant imaginal component seems positively essential to the experience of clamminess. This is indicated by the fact that the same stimulus, which at one time produced a good clamminess and was designated as disagreeable, is described as merely cold, soft and moist when the unpleasant element drops out. This central component, which renders the experience clammy, was elicited much more frequently in the earlier sittings; towards the end of this experiment both clamminess and unpleasantness disappeared from the reports almost entirely. The 16 'clammy' reports of Oz and the 14 of M all appeared in the first 3 series; the 6 of C in the first series; while the 25 of P were distributed throughout the 5 series. P repeatedly asserted, however, that the clammy experiences in the later series were by no means as distinctly clammy as those of the

earlier sittings, and stated that this difference was primarily due to the absence of marked unpleasantness in the later sittings. The other *O*s, all of whom ceased to use the term after a few sittings, indicated that the experiences were no longer unpleasant, implying that some sort of adaptation to these particular stimuli had set in, thereby attesting indirectly and in a negative connection that unpleasantness may be intrinsically bound up with clamminess. The following reports confirm this conclusion, the first one indicating that clamminess drops out during the course of an observation, if unpleasantness is at first realized, and then lost.

"A very disagreeable experience. First a shock of coolness and moistness resulting in a distinct clamminess, but soon there was a reduction of its disagreeable nature. With this reduction of disagreeableness, clamminess was lost, although the coolness, moistness and slight 'giviness' remained throughout" (M). "That was not a good clammy; in other words, it was not at all so unpleasant" (P). "I would hardly call that clammy because it was not much unpleasant" (P). "To be a good clammy there must be an unexpected element of unpleasantness, which usually gives me a sort of creepy feeling" (Oz).

The increased familiarity with the nature of the stimuli probably accounts for the paucity of 'clammy' reports in the later series of observations. One *O* states that when she finds the stimulus to accord with her anticipation of it, or when she takes an attitude which prepares her for the coming stimulation, the clamminess is altered and reduced, apparently through some inhibition of the central imaginal component.

She says, for example: "That one did not seem as clammy as the previous one, but I think it was not due to a difference in sensation, but to a difference in attitude. I was somewhat prepared for this one and it was as I expected it to be" (Oz). "The sensation this time was much like the previous one, yet I did not seem to experience clamminess this time. I think clamminess depends much on the attitude. This experience was familiar, so there was no surprise or shock or novelty" (Oz).

The following report indicates that clamminess does not belong exclusively to determinate kinds of stimulus-objects; that is, it may be realized when an unpleasant suggestion accrues to the sensory data of cold, soft and moist, the stimulus-object to which these characteristics belong being quite unknown.

"Clamminess is based on cold, moistness, and yielding softness, the perception of form and the meaning of not knowing just what it is. If I had my eyes open and could see, I feel that I might not call it clammy" (Oz).

It is not justifiable to assume that this central factor constitutes the whole or all-important factor of the perception, for in that event the reports should distribute themselves uniformly among all 4 of our stimuli. The realization of the perception depends upon the presence both of the sensory data pointed out above and of the central unpleasantness.

Softness. Softness, in so far as our analysis went, was indicated as consisting exclusively of a pattern of pressure qualities, usually of fairly low or moderate intensity, whose extent increases by a gradual spreading simultaneously with a gradual increase of intensity. At a low level of intensity the pressure elements in the softness lack compactness, but at higher intensities of softness the pressure pattern is described as possessing greater compactness. The yielding or "givy" characteristic is ascribed to the gradual temporal integration of changes of extent and of intensity. Softness appears, consequently, to be an integration of 3 attributes of pressure, viz., extent, intensity and duration. Softness also lacks a definite peripheral contour, and sometimes is described as being more compact at the center than at the margin. Softness was synthetized by the application of a jet of air under steady pressure to the tip of *O*'s finger. This was reported by all *O*s as giving rise to typical softness. Yielding softness is aroused by running water from a faucet if the finger is held across the mouth of the faucet.

(5) By this time we were fairly convinced that this central imaginal component is one of the compulsory conditions underlying the perception of clamminess. However, we altered our type of stimuli and instructions somewhat in another series of experiments in order to make certain, now that all *O*s had become acquainted with the clammy experience, whether or not this perception could be evoked under instructions which set up a strict attitude of observing the purely sensory qualities of the experience. We presented moistened bread crumbs or oatmeal in a receptacle to the tips of *O*'s fingers, which extended over the edge of a table. We thus hoped to eliminate the perception of a hand form, which resulted from the stimuli of the last experiment, while leaving the nature of the substance practically unchanged. The instructions read: "After a 'ready' signal a stimulus which will set up a tactual perception will be applied to your finger tips. Observe carefully all touch qualities that are aroused on the fingers and at the end of the stimulation report them in the order of their appearance."

Here, amid a great abundance of reports of cold, moisture or dampness, and a "givy" or yielding pressure or softness—the sensory components of clamminess—we obtained a total of only 5 'clammy' reports, all of which were given by one *O*, P, at a single sitting. In every instance the reference to clamminess was delayed; *i.e.*, it appeared only after all of the strictly sensory qualities had been enumerated. Usually it was a casual remark at the end of the report, as: "I guess I would call that clammy," or "It was a bit clammy." It appears, then, that when the *O*s take the strict attitude of observing the sensory qualities of touch very few 'clammy' references enter the reports, even though all of the sensory components of the clammy experience are represented and reported.

(6) In this experiment we undertook to extend our study of the central component of the clammy experience. Our *O*s had stated that, unless unpleasantness accrues to the requisite

sensory data, the perception of clamminess is not elicited. We wished to determine whether the perception is evoked by the unpleasantness that is associated with specific stimulus-objects or is aroused in connection with any unpleasantness whatsoever that may be added to the adequate sensory data of clamminess. We prepared a series of 10 brief legends, every one of which was suggestive of a more or less unpleasant experience. These legends referred to a number of different kinds of stimulus-objects. *O* was instructed: "After the 'ready' signal *E* will read a brief legend, at the completion of which a stimulus which will arouse a tactual perception will be applied to the volar surface of the palm and fingers. Observe the experience carefully, and at its termination name it and report in detail upon it." As stimuli we used a man's kid glove, a woman's kid glove, and a closely woven muslin bag about 5 cm. by 11 cm., all of which were filled with moistened oatmeal and kept cool in an ice-box. The legends are given below in the order in which they were read to every *O*.

(1) Mr. Jones hastily opened the tool box of his Ford, and was just in the act of picking up some tools when he observed a snake coiled up in the box.

(2) Mr. Smith returned to his home for dinner, walked up to the chair on which his wife was seated, and was just about to kiss her, when he noticed that she was dead.

(3) The cook was carrying a pail of raw oysters when she tripped and fell and immersed her entire right hand in the contents of the pail.

(4) Bill was always careless at the table and was living true to form when he accidentally dropped his spoonful of mayonnaise dressing on the wrist of the friend beside him.

(5) James is one of those mortals who always want to do things that rather disgust their comrades. Recently he has developed the habit of amusing his friends by allowing snails to crawl about on his open hand.

(6) It was a cold icy morning and John had just carried the cuspidor out of his office to empty it. He lifted off the lid and just as he was about to empty the vessel he slipped and his whole hand was thrust into the cuspidor contents.

(7) Mr. S. stopped at a little village to spend the night and inquired at the only inn in town for accommodations. The innkeeper told him every room was taken, but Mr. S. insisted that he must have a place to sleep. Finally the innkeeper stated that one room had only one occupant and that he might sleep with this man provided he promised not to make a light in the room. Mr. S. agreed and retired in the dark. The next morning when he awoke he was horrified to discover that he had slept all night with a corpse.

(8) John came home at noon, found his wife lying on the bed, took hold of her hand, and found that she was dead.

(9) Two strangers occupied the same bed one night because of limited quarters. The next morning one man awoke, and picked up a small round object from the bed. The other interrupted hastily: "Here, give me my glass eye."

(10) Two roommates were always playing pranks on one another. Once upon a time the one decided to leave college late at night before the other had come in without notifying him that he was leaving. But before leaving he brought in a small statue and placed it in the double bed which

the two boys occupied in common. Eventually the other boy came in and retired and was much disconcerted when he discovered that his sleeping mate was lifeless.

The muslin bag was used in every case after the reading of Legends 1, 3, 4, 8 and 9; the two kid gloves were interchanged irregularly after the reading of the other 5 legends.

The following Table indicates the number of times and in connection with what legends the *O*s reported clamminess.

TABLE 1

O / Legend	1	2	3	4	5	6	7	8	9	10
C	*	*	*	*		*	*	*		*
M		*		*	*	*	*	*		*
Oz		*					*	*		
P	*	*	*			*	*	*		*
S	*	*					*	*		

It appears that rather specific kinds of associative connections predispose all *O*s to realize clamminess. Thus, in the allusions to corpses in Legends 2, 7, and 8 every *O* reported clamminess; and in these particular reports every *O* to a greater or less extent expressed an immediate judgment that the experience was distinct clamminess. All *O*s stated, moreover, that they associate clamminess with corpses. Two *O*s, who had been service men and had had unpleasant experiences with corpses, expressed very pronounced associations in connection with these distinctly clammy experiences. But one of them, S, although the same stimuli were used throughout, reported clamminess only one other time than in these three corpse-connections,—that of the snake legend. Legends 1, 6 and 10 were capable of suggesting clamminess moderately consistently, but these references were neither so immediate nor so positive as those pertaining to corpses. In the case of Legend 9, where no 'clammy' reports were given by any *O*, and of 3, 4, and 5, where only occasional 'clammy' reports occur, there was a marked tendency for *O* to assume a 'critical attitude' to the effect that the nature of the stimulus was too discordant with the suggestions of the legend to allow those suggestions to carry over into the perception; hence, if clamminess was mentioned, it was usually in a rather negative connection, as: "That was not much clammy," or "That could hardly be called clammy." In several instances, the stimulus and the legend-suggestion were rather inharmonious, but in spite of this fact *O* failed to shift to this 'critical attitude,' and therefore allowed the suggestions of the legend to carry over into the perception. In such cases the *O*s reported, instead of clamminess, such perceptions as oiliness for Legend 4 and sliminess for Legends 1, 3 and 6.

It should be noted that the perception of definiteness and appropriateness of form is not absolutely essential to the realization of clamminess, since the muslin bag was as effective in securing 'clammy' reports in connection with Legend 8 as the kid glove was in connection with Legends 2 and 7. However, the muslin bag was here perceived as the hand of a corpse. This is perhaps partially due to the fact that the muslin bag aroused the essential sensory qualities of clamminess in connection with a legend possessing strong associative suggestions, and partially to the obtuseness of form-perception by touch when the member remains passive.

(7) In our final work we put a large number of men through the legend-experiment in order to ascertain whether or not the statements of our several *O*s regarding clamminess hold generally. With the exceptions of Professor H. C. McComas and Dr. R. A. Brotemarkle, both members of the teaching staff and both ignorant that the problem was in progress, and Mr. S. C. Dodd, a non-resident graduate student, also ignorant of the problem, these consisted of undergraduates, most of whom had had one or two courses in psychology. The latter were untrained *O*s, only 18 having had as much as the elementary laboratory course. Altogether exactly 100 *O*s served in this experiment. These were divided into two groups of 50 each. Every one of the first 50 gave two observation periods, which as a rule were separated by 2 or 3 days. At the first sitting, we took a series of 10 observations from every *O*, using the same instructions that we employed previously in Experiment 1. At the second period, we used the same instructions and presented the legends in the same order as given above in Experiment 6. The same stimuli—those of Experiment 6—were used at both sittings. At the end of the sittings *E* asked *O* whether the stimuli were 'pleasant, unpleasant or indifferent,' unless *O* had previously volunteered the answer to this question in his report.

In the first series, only 11 of the 50 *O*s reported clamminess. They gave a total of 23 'clammy' references. Five of these men used the term only once, most of them at the first presentation, when a shock of surprise and disagreeableness was aroused. All of these *O*s stated that the stimulus was unpleasant, and particularly so in those instances where clamminess was realized. The 41 *O*s who failed to get clamminess almost invariably reported the stimuli as pleasant or indifferent.

In the second series, with identical stimuli, 35 of the 50 men reported clamminess a total of 148 times, 15 men failing to use the term at all. The influence of the legends is patent, both in the greater total of 'clammy' reports and in the greater number of men reporting the experience. Moreover, in Series 1 the 11 men reporting it averaged only 2 experiences each, while here the

35 *O*s average a little above 4 experiences per series. As appears in the Table below, certain legends seemed distinctly more effective in calling out clamminess than others, and the results confirm our findings in Experiment 6 in a rather clean-cut fashion. The percentages in the Table are calculated from the 35 men reporting the experience, not from the total of 50.

TABLE 2

Legends	1	2	3	4	5	6	7	8	9	10
No. 'clammy' references	15	27	9	6	6	5	27	33	2	18
Percent.	42.8	77.1	25.7	17.1	17.1	14.2	77.1	94.2	5.7	51.4

The 15 men who failed to report clamminess at all were again asked at the end of the second period whether the experiences were 'pleasant, unpleasant or indifferent.' Nine stated that the experiences were indifferent or not at all unpleasant, and 2 that they were rather pleasant. When asked what influence the legends had, all except two of these men stated in substance that they were paying attention to the touch qualities and regarded the legends as irrelevant or perhaps as attempts of *E* to test their ease of distraction. The other two took a positive set against the legends. One said: "I knew you wanted me to let the legends suggest to see how much I would imagine," and the other: "I forced myself against the influence of the legends and did not pay much attention to them." The other 4 men found the experiences unpleasant, but were unable to find the appropriate word; when *E* finally suggested clamminess to them, 3 asserted that it was the word they wanted, but being a rare word in their vocabulary, they were unable to get it; the fourth man did not know the word at all.

Excepting 3 *O*s who use clamminess as synonymous with dampness and moisture, all *O*s who employed the term stated that it is distinctly unpleasant and has fairly definite associative connotations.

The second group of 50 gave only one period to this experiment, the conditions of which were identical with the second sitting of the former group of 50. We thought it possible that clamminess, being a term of rather infrequent usage, might have been reported more frequently at the second sitting because *O* had, perhaps, reflected during the interim upon the previous experiences and searched for a more fitting term than he was able to find at the first sitting, and therefore came back with the word 'clamminess' in mind. But this supposition is invalidated by the fact that in this experiment 38 of the 50 *O*s use the term a total of 156 times, a larger number of reports than the other 50 *O*s gave at their second sitting. The greater number

of 'clammy' references and the greater number of *O*s using the term here may be purely a matter of chance, or it may be due to the fact that a few *O*s in the former work might have used the term at the second sitting excepting for the fact that they recognized the stimuli at the second sitting to be quite similar to those of the previous one, and so took the attitude of reporting the same qualities that they used in the earlier sitting. One or two *O*s gave evidence of trying to recall the qualities they had used the previous hour.

The following Table, which embodies the results of this latter work, indicates again that certain of the legends touch off considerably more clammy experiences than others. The percentages again are based on the 38 men reporting the experience and not upon the total 50.

TABLE 3

Legend	1	2	3	4	5	6	7	8	9	10
No. 'clammy' references	12	29	7	5	10	6	30	34	5	18
Percent.	31.5	76.3	18.4	13.1	26.3	15.7	78.9	89.4	13.1	47.3

Here again Legend 8 receives the highest number of 'clammy' reports. This is perhaps due to the fact that this legend refers explicitly to taking hold of the hand of the corpse, while in Legends 2 and 7 the idea of touching the corpse is merely implicit.

Of the 12 *O*s who did not report clamminess here, 8 took an "objectively critical" attitude and found that the stimulus failed to fulfill the suggestions of the legend. These *O*s found the stimulus too cold, too soft, too wet, not hard or stiff enough, etc. The other 4 blocked off the suggestions of the legends and reported the purely sensory or peripheral qualities due to the assumption of a set against the influence of the legends.

Sliminess. Although we have made neither any attempt to touch off slimy perceptions nor any direct attack upon the analysis of sliminess, there are indications in the reports that this perception too may depend in part upon a central emotional factor. Thus the 50 *O*s in the non-legend observations reported sliminess only 10 times, while in the legend series they report it a total of 55 times. The other 50 *O*s gave a total of 54 'slimy' references. Moreover, there is a fairly definite distribution of the larger number of these reports with the legends that most naturally suggest sliminess. It should be pointed out that the conditions of our work were not particularly favorable to the realization of this perception, since there was no movement of any consequence either of member or of stimulus. But certainly, unless some of the sensory data were represented to which this central factor was added, there was no perception of sliminess. The following Table shows the distribution of the 109 reports of sliminess among the different legends.

TABLE 4

Legend	1	2	3	4	5	6	7	8	9	10
Group 1	10	3	7	3	12	10	3	1	4	2
Group 2	8	2	8	1	14	12	4	2	2	1

The perception of *oiliness* was mentioned 5 times in connection with the mayonnaise legend, but did not appear at all in connection with any of the others. *Slipperiness* and *greasiness* were reported a number of times but show no definite connection with the suggestions of any particular legend.

CONCLUSION

Clamminess is a complex tactual perception, which breaks up under introspective analysis into (1) a sensory or peripheral part and (2) a central or imaginal constituent. The sensory or peripheral part analyses further into sensations of cold and a yielding softness, which must be so combined as to give rise to the simple sensory perception of moisture. Cold is a simple sense quality, but softness appears to be a simple perceptual integration of the intensive, extensive and durative attributes of the pressure quality. The central or imaginal component consists of an unpleasantness, which attaches to associative connotations of rather definite kinds of stimulus-objects. A genuine clammy experience is always designated as distinctly unpleasant.

The perception of clamminess fails to mature, either in the absence of one of the constituent sensory data and when these in combination fall short of setting up the perception of moisture, or in the absence of the central or imaginal factor when all the sensory data fulfill the requirements of that part of the perception. The assumption of an 'objectively critical' or of a 'strictly sensory' attitude renders unlikely the arousal of the perception, since such an attitude blocks out the central constituent; but the assumption of an 'object'-attitude favors the touching off of the experience, owing to the fact that such an attitude facilitates the accrual of the central or imaginal component to the requisite sensory data.

REACTION TIMES AND THE CONSCIOUSNESS OF DECEPTION[1]

By Eva R. Goldstein, U. S. Veterans' Hospital No. 42

Historical Setting

Both Jung (4, 5, 6) and Wertheimer (10, 11) claim the discovery of the association-reaction method of studying mental processes. The method consists essentially of giving to the subject a list of words with the instruction that, as the list of words is presented to him, one by one, he is to answer as quickly as possible by the first word that occurs to his mind in connection with the seen or heard stimulus-word, and of measuring the reaction time to each word. If any of these reaction times is unusually long, such a lengthened reaction is taken to indicate that some emotional factor is at work in addition to the association. The primary interest of the early workers in this field lay in the diagnosis of the emotional complexes underlying cases of hysteria and neurasthenia. The method was, however, also applied by Jung and Wertheimer in attempts to detect crime. In this use of the association-reaction method, the list of words used as stimuli contains words relating to the crime. The reaction time to these critical words was found to be lengthened in the case of those criminals who attempted deception by avoiding responses which implicated them in the crime.

In 1909 Yerkes and Berry (12) and Henke and Eddy (3) published experimental studies which were designed to test the applicability of the association-reaction method to the detection of crime. In these experiments, *S* was allowed privately to open one of two boxes, and *E* endeavored by means of a series of association-reactions to determine which box had been opened. These investigators found that reaction times tended to be longer whenever deception was practised. Leach and Washburn (7) published a careful qualitative and quantitative study of the method in 1910. They used a similar procedure and tried to test the method on as many *S*s as possible. Their quantitative results are in agreement with those of the previous investigations, namely, that during a deceptive situation reaction times are increased. We are not concerned here with the qualitative aspects of the results obtained, that is, with the character of the words with which *S* responds, although the words are equally important with the reaction times in the detection of crime.

Marston (9) published in 1920 an account of a study in which he attempted to test the thesis that reaction times are increased during deception. His method purposed the elimination of the qualitative aspect of the association-reaction experiment, the substitution for this aspect of arithmetical exercises, and the isolation of the quantitative measurements. It aimed at the exclusion of all mental elements extraneous to the interaction of deceptive consciousness and reaction times. The results of his study disclosed two distinct types of reaction time behavior during deception. First, the "positive," clearly evident in 4 out of 10 *S*s, and previously believed to be practically universal, was characterized by increased length of reaction times during deception. Secondly, a "negative" type manifested itself with equal distinctness in 3 *S*s, in whose cases the reaction times appeared to be consistently shortened during deception. In addition there was a mixed group of 4 *S*s who completely shifted from positive to negative on

[1]From the Psychological Laboratory of the University of Pittsburgh (communicated by Jesse H. White and Gilbert J. Rich). The experimental work was performed in the spring of 1921.

different days, but behaved consistently within the positive or negative type in each single day's work. The introspective reports of the positive-type subjects show fear, excitement, and confusion of motor impulses during deception, all of which were lacking in the experiences of the subject of the negative type. From these experiments, Marston concluded that increased reaction times, as well as other symptoms of emotional excitement, can be taken as indications of deception only in those persons who have been shown to be of the positive type, while decreased times and lack of symptoms of emotional excitement can be taken as indications of deception in persons of the negative type.

EXPERIMENT I

Problem.—Marston's results are at variance with the findings of all workers in the psychology of deception previous to him. His conclusion would lead to the belief that the thesis, reaction times are increased during deception, is fallacious. A consideration of his work brings to the front the following questions: Do the conditions of Marston's experiment actually disclose two distinct types of reaction-time behavior, a "positive" type characterized by increased reaction time whenever there is an attempt to deceive, and a "negative" type characterized by decreased reaction times during deception? Was Marston's method adequate for the production of a *deceptive* situation in which the quantitative factors were isolated by elimination of the qualitative aspect? Experiment I. aims to answer these questions.

Observers.—Ten *Os* took part in the experiment. One (S) was an Instructor in the Department of Psychology, a man with his doctor's degree and highly practised in the performance of arithmetical exercises. One (L) was an undergraduate student of psychology without laboratory experience. The remaining eight, six women and two men, were students in the second semester of the laboratory course in psychology.

Apparatus and Procedure.—This experiment was necessarily a repetition of Marston's work. So far as his published account enabled us to do so, every detail of materials, conditions and procedure of Marston's study was duplicated.[2]

320 pasteboard index cards, 8 by 5 in., were prepared, every one of which carried two parallel columns of 10 two-digit numbers each. Every number column or list was repeated 4 times, every time with a different digit to subtract or add, in order to exclude as far as possible any chance influence of combinations of figures peculiarly difficult to any individual *O*. These cards were divided into 40 sets of 8 cards each. The cards were presented to the *O* in groups of 8 each, blank side up. The following instructions were given.

"You will be given a group of eight cards, upon each of which are written two parallel columns of ten two-digit numbers each. These cards will be placed blank side up before you. At the signal 'ready' you are to turn over the top card and at the signal 'go' you are to add to or subtract a given number from each number in one of the lists, as directed, giving the answer as fast as you can. Before the giving of the 'ready' signal you will

[2]In a conversation following the completion of this portion of the work, Marston indicated that his experiments were performed in the presence of a 'jury.' Since he gave no indication of the presence of a 'jury' in his printed account, this condition was not duplicated by us.

be directed to choose either the right or the left column and to proceed through that column either up or down, adding or subtracting from each number in the list 2, 3, 4, or 5. In any four out of each group of eight cards you are to reverse the instructions by taking the opposite list, by proceeding in the opposite direction, and by adding when instructed to subtract and *vice versa*. You are to deceive me as to the identity of the cards with which you reversed the instructions by proceeding to make your calculations with the list on these cards as rapidly as on the cards with which you obeyed the instructions.

"At the completion of each card you are to enter in the table provided by the symbols C (correct) and O (opposite) whether you obeyed the instructions or disobeyed them.

"You will then characterize with as much detail as possible your attitude when obeying directions and your attitude when disobeying directions."

Reaction times were taken with a stop-watch showing fifths of a sec. Time was allowed between the instructions and the starting signal for the *O* to perform the central (mental) process of reversing the instructions, and the watch was started simultaneously with pronouncement by the *O* of his first result, so that the reaction times recorded actually included the time used in 9 instead of 10 additions or subtractions.

The instructions for every *O* and with every set of cards continued as follows.

"Take the column to the right; proceed up; add 2.
Take the column to the left; proceed down; subtract 3.
Take the column to the right; proceed up; add 4.
Take the column to the left; proceed down; subtract 5.
Take the column to the left; proceed up; subtract 2.
Take the column to the right; proceed down; add 3.
Take the column to the left; proceed up; subtract 4.
Take the column to the right; proceed down; add 5."

At the completion of every group of 8 cards, *E* attempted to judge by means of the reaction times alone which 4 lists the *O*s had performed according to instructions (C) and which 4 they had performed in the way opposite to the directions (O).[3]

The series of 40 sets of 8 cards each were used with every *O* except L. In L's case, 47 sets were used. The amount of work done in a single experimental period consisted of from 3 to 16 sets. An experimental period varied from one to two hours in length.

Results.—The average reaction times for each *O* revealed two types of response under the experimental situation. One of these was analogous to Marston's positive type, and was characterized by longer reaction times for the cards with which the instructions were disobeyed; the second type was analogous to Marston's negative type, and was characterized by shorter reaction times for the cards with which the instructions were disobeyed. Three *O*s were of the positive type, 7 of the negative

[3]The percentages of correct judgments by *E* varied so little from 50% that their significance is negligible and they are omitted here. They are reactions of *E*, not of the *O*s, and are, therefore, irrelevant to our problem. Their percentages may be found in a bound manuscript with the title "Reaction Times During Deception" which is in the University of Pittsburgh Library and may be borrowed under the usual courtesies of interlibrary exchange.

type. The significance of these types is to be understood only by analysis of the numerical and introspective data for the separate *O*s.

Table I. shows the average reaction times of the three positively inclined *O*s to the cards with which they obeyed instructions ("C" reaction times); their average reaction times to the cards with which they disobeyed instructions ("O" reaction times); the difference between these two averages; the probable error of this difference; and the probable correctness of the difference, computed according to the formula suggested by Boring (1).

TABLE I

Observer	Average "C" Reaction Times	Average "O" Reaction Times	Difference between "O" and "C" Averages	Probable Error of Difference	Probable Correctness of Difference
E	6.4095	6.4412	.0317	.2671	.5550
H	8.6550	9.0312	.3762	.1572	.9125
Mc	8.3175	8.3812	1.0637	.1409	.6150

An analysis of the introspections[4] of these three *O*s shows that they were actually conscious of deceiving whenever they disobeyed instructions. The consciousness of deception appeared in various guises.

(1) It made the *O*s feel guilty when disobeying directions.

E, 1. "When I disobeyed I seemed to feel that it wasn't right."

H, 4. "I realize all the time I am disobeying that I am disobeying. There is always a strain or tension of my whole body while disobeying."

Mc, 4. "At first when I decided to disobey instructions I felt like a liar."

(2) It made them self-conscious when disobeying directions.

E, 1. "I felt more self-conscious when I disobeyed than when I obeyed."

E, 2. "This time, when I disobeyed instructions, I seemed to hesitate. This hesitation was caused by self-consciousness, because when obeying I felt that I wasn't deceiving the experimenter."

H, 2. "This time I seemed doubly conscious that I was disobeying."

(3) It was harder to disobey.

E, 1. "It was harder to disobey instructions."

H, 2. "As usual it is much harder for me to disobey."

H, 4. "It is much easier to obey."

(4) It made them hesitate when reversing instructions.

E, 1. "I hesitated longer when I disobeyed than when I obeyed the instructions. When I disobeyed I seemed to feel that it wasn't right and this led to a hesitancy."

[4]Each quotation from an introspective report is prefaced by the initial of the *O* and by the day upon which the report is made, as 1 for the first day of experimentation, 2 for second day of experimentation, etc.

E, 2. "This time when I obeyed instructions I seemed to hesitate. This hesitation was caused by self-consciousness, because when obeying I felt that I wasn't deceiving the experimenter."

H, 1. "This time I hesitated and stopped at intervals when disobeying and was more conscious of the fact that I was disobeying, and was more excited. I didn't go so smoothly as before."

(5) It caused strain and tension when disobeying instructions.

H, 4. "There is always a strain or tension of my whole body while disobeying, for when the command is given it means to reverse the method on a second's notice."

Mc, 2. "I have a greater feeling of strain when I disobey."

(6) It caused a feeling of relaxation and relief when obeying.

H, 4. "It is much easier to obey. I feel relaxed and would rather disobey the first four times and then be able to relax while obeying."

Mc, 1. "I felt almost relaxed when I was obeying."

Mc, 4. "After I have decided to obey or disobey the instructions, I have a feeling of relaxation and pleasantness."

(7) It caused excitement when disobeying the instructions.

H, 1. "When I know that I am going to obey I feel relaxed and relieved and I go with much more speed."

H, 2. "I get excited and must concentrate whenever I disobey."

M, 1. "I felt more excited while disobeying than while obeying."

M, 4. "At first when I decided to disobey instructions I felt like a liar. I had an excited, unpleasant feeling."

The consciousness of deceiving was evident, even when the *O*s attempted to devise a plan whereby to make equal the reaction times for the C and O cards.

H, 2. "By trying to mix my methods so as to change, I do get excited and must concentrate. However, I have a fixed time when I shall disobey, but as usual it is much harder for me to disobey. It is much easier to add than to subtract; so in disobeying, I tried to reverse the method and to add. But in so doing, I hesitated."

H, 4. "My plan when directions are given is to trick the experimenter· I reverse and change in order that my method may be different with each set of cards, so that the experimenter may not know when I am obeying or disobeying. I found myself hesitating while disobeying; so, in order that the experimenter might not learn my method, I hesitated at times when obeying. Even then I realize that I am disobeying. It is much easier to obey."

The consciousness of deception bears a definite relation to the probable correctness of the difference between the "O" and the "C" reaction times. The more acutely conscious the *O* was of deceiving, as shown by his introspective reports, the more probably correct was the positive difference between the times of the two forms of response. Thus, on the basis of the degree to which they were conscious of deceiving, the three *O*s would be ranked in the order: H, Mc, E. They stand in the same order with respect to the probable correctness of the difference in reaction times.

The quantitative data for the 7 negatively inclined *O*s are shown in Table II. The introspections of these *O*s reveal that they were not conscious of deceiving.

A, 3. "There is no difference in my consciousness now, so far as I can judge, when I obey or disobey instructions."

B, 2. "I did not feel any difference between obeying and disobeying."

D, 3. "After I have started, the nature of my consciousness is not affected by my decision to obey or disobey."

D, 6. "My mental attitude when I obey is no different from what it is when I disobey."

L, 1. "I have no special feeling attitude either way."

L, 4. "Whether or not I obey does not affect my consciousness, so far as I know."

M, 1. "I do not feel affected by the decision to obey."

R, 5. "As a rule, the decision one way or the other did not affect the nature of consciousness."

S, 2. "There is no difference in my mental attitude between the times when I obey and when I disobey. I simply decide upon what I shall do and then do it."

It will be remembered that the actual reaction times recorded included only 9 additions or subtractions, and did not include the time for the central (mental) process of reversing the instructions. The central process took place between the 'ready' and the 'go' signals. Nor was the time for the first calculation by the *O* included in the reaction time recorded. It was during this time, if at all, that the decision to obey or disobey had any affect upon the *O*s. The decisions once made, however, the experiment became simply a process of performing some arithmetical exercises.

TABLE II

Observer	Average "C" Reaction Times	Average "O" Reaction Times	Difference between "C" and "O" Averages	Probable Error of Difference	Probable Correctness of Difference
A	11.9750	11.0775	.0975	.1800	.6350
B	13.7675	12.4862	1.2813	.2420	1.0000
D	15.3862	14.9662	.4200	.3331	.8000
L	11.4361	10.5989	.8372	.2376	.9875
M	13.5556	12.6557	.8781	.1890	1.0000
R	9.9925	9.9175	.0750	.1983	.5575
S	5.9437	5.8750	.0687	.0739	.7300

B, 4. "Once I had decided whether I should obey or disobey, I tried to get through as quickly as possible. Which way I had decided did not affect me."

B, 5. "I disobey and obey according to some arrangement of how the C's and O's will appear in a list, varying the arrangement from time to time. I decide to obey or disobey before I turn over the card. In some instances, however, I changed my mind after turning the card over. This change was due to the order in which the numbers appeared. Sometimes after seeing the numbers it seems as though it would be easier to disobey than to obey and then I did that. My consciousness is not affected after I have decided what to do."

D, 3. "I decide to obey or disobey sometimes in a regular order before I begin the set of cards. I always decide before I hear the instructions. If I decide to disobey instructions, I am under somewhat of a strain until I am sure that I have reversed the directions correctly. After I have started the nature of my consciousness is not affected by my decision to obey or disobey."

L, 4. "I decide to disobey instructions always just after hearing the process named. Whether or not I obey does not affect my consciousness."

R, 2. "In deciding, there is some confusion of consciousness; the two sides seem to be contending for themselves. I feel more certain and satisfied when I have reached a decision."

R, 3. "When making a decision there was much confusion in consciousness, and it was usually necessary to exert some effort in order to reach a decision and to remember it so as not to get mixed up. When a decision was once reached, I seldom really got it mixed up, but had to concentrate on it. As a rule, the decision one way or the other did not affect the nature of consciousness."

All the *O*s reported a plan, made for the purpose of equalizing the reaction times on all cards. The plan, in general, consisted in setting a medium pace at which to call off the calculations for both the C and the O cards; in going slower on the C cards; and in arranging the order C's and O's in such a way as to be able to disobey with the easier process.

A, 1. "I hurried on the opposite cards and went slower on the correct ones."

B, 3. "I tried always to add when disobeying so as to go faster. I tried to increase the speed when disobeying."

B, 4. "When disobeying, I nearly always added a 2, because I found that it was easier to do so and that adding 2 went faster. I tried to disobey whenever the instructions were to add or subtract 5."

D, 3. "I prefer to begin obeying, because the fuss of getting started prevents my disobeying on the first set. I like to get the four times I must disobey off my mind as soon as possible."

L, 1. "I consciously selected all requests to subtract for the cards on which I should disobey."

L, 2. "I try to pick the easiest alternative and keep within the rules."

L, 3. "I prefer to add anything over 3."

L, 4. "It seems easier to take four C's and then four O's."

R, 1. "By making an extra effort at speed when disobeying and hurrying less when I obeyed, I hoped to make the times for all cards as nearly equal as possible."

S, 4. "I usually choose to disobey the more difficult instructions and to obey the easier instructions."

The *O*s felt no strain and tension when disobeying, and did not experience a feeling of relief and relaxation when obeying. Nor did they feel any excitement when disobeying.

B, 3. "I felt more confidence when disobeying and tried to gain speed from that fact."

D, 4. "When I put out of my consciousness the idea of obeying or disobeying and of deceiving the experimenter, and simply performed the process as I had decided in my own mind, I found it easier for me and less revealing to the experimenter. That is, I get rid of the ethical feeling and work on a mechanical basis."

D, 6. "The process is entirely mechanical, involving not the least feeling of moral obligation."

Thus, in the case of the 7 *O*s, whose quantitative measurements showed a decrease in reaction times for cards on which they reversed instructions, there was no consciousness of deception. The decision to obey or disobey was made either before or after the directions how to proceed were given; and if this decision did affect the consciousness of the individual *O*, it did so for only one out of the ten operations; and the time for this operation was not included in the reaction times recorded. Therefore, there was no means to determine whether the desicion to obey or disobey caused the reaction time for the first calculation to be decreased or increased. But, usually, once the decision was made, all idea of obeying and disobeying was dropped, and the performance of the arithmetical exercises became merely a matter of adding and subtracting. They felt no strain or tension and excitement when disobeying; nor did they feel any relief and relaxation when obeying. It made no difference to them whether they obeyed or disobeyed.

Further analysis of the quantitative results discloses the fact that every *O*, whether positive or negative, had at least one reversal day, that is, one day or more on which he completely shifted from his ordinary attitude. Thus, every positive *O* had at least one day on which he was negative, and every negative *O* had at least one day on which he was positive. When these reversals were checked up with the introspections of the *O* on reversal days, it was found that on negative reversal days of positive *O*s the introspections were similar to those of the negative type, and that on positive reversal days of negative *O*s the introspections were similar to those of the positive type.

TABLE III

Observer	Number of Sets a Day	Average "C" Reaction Times	Average "O" Reaction Times	Difference between "O" and "C" Averages	Probable Error of Difference	Probable Correctness of Difference
E	6	8.0250	7.2000	− .8250	.1944	1.0000
	6	7.2333	6.7166	− .5167	.1903	.9700
	16	6.2593	6.3625	+ .1032	.1014	.7500
	12	5.6750	6.2625	+ .3566	.1181	.9800
H	4	11.4750	11.3500	− .1250	.6480	.5700
	12	9.7958	10.4000	+ .6042	.1596	.9900
	12	7.6666	7.8250	+ .1584	.1937	.7085
	8	7.6375	7.8000	+ .1625	.2414	.6875
	4	7.4750	8.6375	+1.1625	.4181	.9700
Mc	8	10.2812	10.8750	+ .5938	.2810	.8500
	8	8.4375	8.3000	− .1375	.2246	.6500
	8	8.2000	7.7687	− .4313	.2454	.8800
	16	7.3500	7.4812	+ .1312	.1398	.7400

In Table III.[5] are shown the data for the three positive *Os* fractionated by periods of experimentation (which occurred on separate days). It will be noted from this Table that the first and second periods of experimentation were reversal days for E, that is, days upon which the reaction times were decreased for the cards with which she disobeyed instructions; and that the second and third days were reversal days for Mc. Thus, the two *Os*, for whom the probability that the positive difference was not due to chance was slight, were actually positive in only half of the experimental periods. Moreover, the probability that the positiveness or negativeness of each of these *Os* on any given day was not due to chance was in most cases relatively high. On the other hand, H, who was markedly positive, had only one reversal day (the first period of experimentation), and in this case the probable correctness of the negative difference was so low as to show that chance factors were operative,[6] and indicates that tendencies in both directions were operative during that period. This conclusion is borne out by the fact that of the 4 sets of cards completed during the experimental hour, 2 were negative and 2 were positive. The introspective reports show a difference in attitude between the two types of reaction. The introspection for the positive set was as follows.

H, 1. "When I know that I am going to obey, I feel relieved and relaxed and I go with much more speed. In disobeying, it is harder to get adjusted and to concentrate and I am much slower in my responses. I hesitated and stopped at intervals when disobeying and was more conscious of the fact and was more excited. It didn't go smoothly. There was more of a strain this time."

Whereas the introspection for the negative set was as follows:

H, 1. "This time, although I hesitated in subtracting, I have no difficulty in disobeying. In fact, it seemed just as easy to disobey as to obey, compared with the previous time." (The previous time, the set was positive.)

A few excerpts from the introspections of the two other *Os* on the reversal days will show that, in general, their consciousness while disobeying was similar to that of the 7 *Os* of the negative group.

E, 1. "During this experiment, I obeyed or disobeyed according to the instruction. If told to add, I added; but when told to subtract I disobeyed so that I could add. A few times I broke from this general rule just in order to try what the result would be. Heretofore, I believe that I have accelerated when disobeying in order to cover up the fact, but now I try to keep about the same time for all, sometimes trying to go faster when I obey than when I disobey. I perform this work without consciousness of deception. The most interesting and most prominent motive is one of speed. The idea of really trying to deceive does not enter my mind."

[5]The differences between the "C" and the "O" averages are called positive whenever the "O" times are greater than the "C" times, and negative whenever the "C" times are the greater.

[6]It must be remembered that a difference between averages which is due to chance factors alone would have a probable correctness of .50.

E, 2. "I find that the factor that caused me to obey or disobey instructions most of the time was based upon the fact that I like to add better than to subtract. Therefore, I added every time I could. By disobeying when told to subtract, I added nearly all the time. However, I wouldn't always carry this out; at such times I would decide before the instructions were given. Several times I deliberately decided before the experiment began. I know that I have to obey four times and disobey four times; so I make up my mind as to what I am going to do and I no longer have any feeling of guilt when disobeying. I do not believe that the decision to obey or disobey has any effect upon the time in which one calls off the figures. I think it all depends upon the easiness or difficulty of the column of figures. It also depends upon whether one is interested or not interested in performing the process."

Mc, 3. "I feel relaxed when it is all over. When I finish the series either of obeying or disobeying, I feel about the same degree of pleasantness and relaxation."

Table IV. shows the data for the 7 negative *O*s, fractionated by experimental periods. A few selections from the introspections of this group on their reversal days suffice to show that on these days they were conscious of deception very much as were the positive *O*s, as described above.

A, 2. "The answers came much faster when adding or subtracting 2. Several times I think I made mistakes and knew them, but I didn't correct them. I find always more difficulty in doing subtractions than in doing additions. I tried this time to go very fast, giving as short a space of time before the first number in all eight cases as possible. On the fourth one, I mixed the cards and after the signal was given I had to straighten them. It took longer to react. I rushed through them all wildly and tried to give accurate judgments. I found it easier to make quick reactions because of the method of grouping the C's and O's. I tried going slower on all judgments. I went very fast. I wondered all the time whether I was fooling the experimenter this time, since the last time I had gone clear wrong."

B, 1. "When trying to gain speed when disobeying, I became somewhat confused. I sometimes became confused over the number to be subtracted or added. There was more confusion when disobeying than when obeying. There was less confusion as I became accustomed to adding or subtracting. I found that I could add faster than I could subtract. Trying to increase the speed did not cause so much confusion this time. I became more accustomed to the numbers and the confusion still lessened. When disobeying, I always added as that seemed to go faster. I gained speed because the confusion lessened."

D, 1. "There is increasing difficulty in getting started when I disobey instructions. It is easier for me to add than to subtract. It is easier to add or subtract very small numbers than those a little larger. There is considerable strain when starting in to reverse instructions."

L, 6. "I was altogether worried when making the decision as to obedience. I felt somewhat guilty when disobeying."

M, 2. "I am under a greater strain when disobeying than when obeying, and feel that I am unable to act normally and proceed with the same regularity as when obeying. When the card is completed, there is a slight relaxation, but as new instructions follow quickly, attention and the plan of procedure cause a renewed effort to be made immediately."

TABLE IV

Observer	Number of sets on Day	Average "C" Reaction Times	Average "O" Reaction Times	Difference between "O" and "C" Averages	Probable Error of Difference	Probable Correctness of Difference
A	4	18.1375	16.4125	−1.7250	.7688	.9475
	10	12.0600	12.1150	+ .0550	.3050	.5600
	14	11.6678	11.2607	− .4071	.2063	.9125
	12	10.2125	10.8875	+ .6750	.1749	.9950
B	12	13.6541	14.0166	+ .3625	.4830	.6950
	12	14.2625	12.5833	−1.6742	.3648	1.0000
	16	13.4812	11.2637	−2.2175	.3628	1.0000
D	3	16.2000	18.4833	+2.2833	1.1874	.7500
	9	14.7916	14.7333	− .0583	.4913	.5350
	4	15.9745	13.9875	−1.9870	.7952	.9600
	4	15.1125	14.8500	− .2625	.8416	.5850
	8	16.4000	14.5187	−1.8813	.4396	1.0000
	12	14.8458	14.7166	− .1292	.3693	.5497
L	7	14.7285	12.3357	−2.3928	.6600	.9925
	9	12.6111	12.0308	− .5803	.4073	.8450
	8	12.9062	10.3500	−2.5562	.7270	.9900
	7	9.5428	8.7000	− .8428	.3311	.9575
	8	9.7812	10.2750	+ .4938	.3536	.8300
	8	9.0750	9.7062	+ .6312	.3573	.8900
M	12	14.5000	13.4082	−1.0918	.3584	.9800
	8	13.0281	13.0875	+ .0594	.3498	.6250
	4	14.8750	12.9375	−1.9375	.4397	1.0000
	4	15.9750	13.7125	−2.2625	.6130	.9925
	12	11.7166	11.2416	− .4750	.2684	.9375
R	8	12.8000	11.9187	− .8813	.5965	.8400
	8	8.7625	10.7625	+2.0000	.3510	1.0000
	8	9.2250	8.7312	− .4938	.3507	.8250
	12	9.4208	9.1375	− .2833	.3262	.7200
	4	10.8750	8.9375	−1.9375	.4752	1.0000
S	8	6.6062	6.8375	+ .2313	.1939	.7900
	8	5.8125	5.7312	− .0813	.1486	.6380
	12	6.0000	5.5375	− .4625	.1171	.9900
	12	5.5333	5.6660	+ .1327	.1300	.5600

R, 2. "During the cards with which I obeyed I had but few conflicts. When I changed to disobedience I found somewhat of a predisposition toward obedience. It seemed more of an effort to disobey and I was anxious for the times when I could obey. Obeying came as a relief. I believe I made better time on obedience than on disobedience."

S, 1. "There was a feeling of confusion about remembering whether I am obeying or disobeying instructions. There was a feeling of confusion about getting the right instructions and remembering whether I had determined to obey or disobey."[7]

[7]In the case of S, the difference in attitude on the reversal day was marked. It must be remembered that he had had considerable experience with the performance of arithmetical exercises and the experiment became for him a mechanical procedure. He was unaware of deception, yet showed a shift of attitude on his reversal days.

Thus a detailed analysis of the introspections reveals the fact that, on the days on which the reaction times for the O-cards are decreased, the positive-type *O*s are not aware of any difference in their attitude when disobeying as compared with their attitude when obeying; whereas on the days on which the reaction times for the O-cards are increased the negative-type *O*s are conscious of deception,—this consciousness of deception appearing in various guises, as a feeling of guilt or as a feeling of strain and tension and excitement when disobeying, and as a feeling of relief and relaxation when obeying.[8]

Conclusions.—Under the conditions of this experiment, two tendencies in reaction time behavior appeared. The one tendency consisted in the reaction times being longer for the cards for which the instructions were reversed. This type of reaction, whether it appeared in the majority of an *O*'s responses or in the responses on a single day, represented a true deception type, for the *O*s reported, when they were reacting in this fashion, consciousness of deception, strain, conflict, and emotional disturbances when disobeying instructions. Following Marston's nomenclature, we have called this the positive type of response.

The other tendency was characterized by shorter reaction times for the cards with which the instructions were reversed. We have called this the negative type. It was not a true deception type. The introspective reports of *O*s responding in this manner showed that they were not conscious of deception, strain, conflict, or emotional disturbances when disobeying instructions. How, then, did this form of response appear during the experiment? The experimental procedure, devised by Marston with the intent to isolate the quantitative phase of deception, simplified the situation to which the *O* reacted to such an extent that all meaning of deception was, in the majority of cases, eliminated from the symbols dealt with in the *O*s' mental processes. The experiment, as a result, became at these times nothing else than practice in arithmetic, into which the factor of deception did not enter. That 7 of the 10 *O*s (and the remaining *O*s on certain days) did not show longer reaction times when disobeying instructions may not be taken to indicate increased reaction times during deception, for a condition of deception was not induced in these cases by the experiment. The so-called negative type is not a type of response to deception, but a type of non-deceptive response to this particular experimental situation.[9]

[8] An analysis of the relative frequency of positive, negative, and indifferent sets for every *O* for every day's work may be found in the manuscript copy mentioned in Footnote 3.

[9] Marston assumed that his experimental setting induced deception because the instructions were disobeyed. We have cited introspective reports to prove the incorrectness of this interpretation.

If the factor of deception did operate in any way for the *O*s of the negative type, it did so only during the process of deciding to disobey the instructions, of actually making the reversal in the instructions, and (perhaps) of making the first calculation on the card. Thereafter, the mental set induced at the beginning of the card gave the cue to the individual computation. But the time required for making that decision, for beginning its operation, and for making the first calculation was not included in the reaction times measured, and there was, thus, no quantitative measure of the effect of disobedience upon these initial phases of the reaction to the card.

Moreover, the wording of the instructions "to make your calculations with the list of these ["O"] cards as rapidly as on the cards with which you obeyed instructions" implied that the reaction times for the "C" cards would naturally be smaller than the reaction times for the "O" cards; and on the basis of this implication the *O*s formed their plans to make the times equal for all cards. Thus, they invariably slowed down on the "C" cards and hurried on the "O" cards.

To summarize: a repetition of Marston's experiment has failed to give evidence of two types of reaction-time response to deception; but it has shown that the experiment induces deception in some *O*s and at some times, and fails to induce deception in other *O*s and at other times, the failures to produce deception being nore numerous than the successes.

Experiment II

Problem.—Experiment I. failed to produce, save in a few instances, deception on the part of the *O*s, and therefore affords inadequate information concerning the effect of deception upon reaction times. In the second part of the work, we have attempted so to alter the procedure as to set a new experimental situation which would be adequate to induce deception in the *O* and would at the same time be so simple as to isolate the quantitative phase of the deceptive response from complicating factors.

The most significant change in the situation consisted in extending the effects of the decision to obey or disobey to all items on the stimulus-card. In Experiment I., the same arithmetical operation was performed on every item on the card; while in Experiment II. the operations of addition and subtraction were distributed throughout the card and the *O* had either to obey by performing the *various* indicated operations or to disobey by performing the opposite of the *various* indicated operations. A decision to disobey by adding to (or subtracting from, as the case might be) all items on the card could not be made by the *O* at the beginning of the card and then forgotten, allowing the mental set induced to give the cue for every computation. As

the decision to use one or the other column and to proceed either up or down likewise affected only the beginning of the response, only one list of exercises and one direction of procedure were used.

A further change was the substitution of instructions "to make your calculations on these cards ["O"] at the same rate as on the cards where you performed the operations indicated" in place of the former instructions "to make your calculations on the cards ["O"] as rapidly as on the cards with which you obeyed the instructions." It was hoped to avoid by this change the suggestion to the *O* that he would go faster on the "C" cards then on the "O" cards, as some *O*s had regulated their pace in accordance with this suggestion. The suggestion from Experiment I. may, of course, have held over for some *O*s.

Observers, Apparatus, and Procedure.—The *O*s in Experiment II. were the same as those in Experiment I.

320 pasteboard index cards, 8 by 5 in., were prepared, every one of which carried a single list of 10 arithmetical exercises. Every exercise consisted in subtracting or adding a single digit number to a two-digit number. There were 5 additions and 5 subtractions on every card. The number to be subtracted or added was constant for every card, and was either 2, 3, 4, or 5. The cards were divided into 32 sets of 10 cards each.

The instructions were as follows.

"You will be given a group of ten cards, upon each of which are written ten arithmetical problems. These problems involve addition and subtraction. These cards will be placed blank side up before you. At the signal 'ready,' you are to turn over the top card, and at the signal 'go,' you are to proceed through the problems on the card, giving the answer to each problem as fast as you can. In any five cards out of each group of ten cards you are to perform the operations indicated on each card and in the other five out of that group you are to reverse the operations indicated; that is, you will subtract instead of adding and add instead of subtracting. You are to deceive me as to the identity of the cards where you reversed the operations indicated by proceeding to make your calculations on these cards at the same rate as on the cards where you performed the operations indicated.

"At the completion of each card you are to enter in the table provided by the symbols C (correct) and O (opposite) whether you performed the operations as indicated or reversed them.

"At the conclusion of each set of ten cards you will be shown the experimenter's judgment as to the cards on which you reversed the operations indicated.

"At the end of the hour you will give an introspective description of your consciousness when performing the operations as indicated and when reversing them, noting especially any and all points of difference between the two."

In order actually to provide a motive for deceiving, which motive was lacking in Experiment I., every *O*, with the exception of S, was informed that his semester's grade in the course which he was taking in Psychology would be raised in accordance with the degree to which he succeeded in lowering the percentage of correct judgments by *E* as to when he was disobeying instructions.[10] The *O* was warned that he must obey the directions for the experiment.

[10]No *O* was able to bring this percentage for him below 50% and thereby obtain an increase in grade.

Reaction times were taken with a stop-watch reaching to fifths of a sec. Time was allowed between the giving of the 'ready' and 'go' signals for the *O* to perform the central (mental) process of deciding to obey or disobey, and the watch was started simultaneously with the pronouncement by the *O* of his first result, so that the reaction times as recorded include the time for 9 of the 10 arithmetical exercises.[11]

At the completion of every set of 10 cards, *E* attempted to judge, by means of the reaction times alone, on which 5 cards the *O* had performed the operations as indicated (C) and on which 5 cards he had performed the opposite operations (O).[12]

All of the *O*s completed the series of 32 sets of 10 cards each. The amount of work done in a single experimental period was from 4 to 10 sets. An experimental period varied from one to two hours in length.

Results.—Analysis of the quantitative measurements of Experiment II., as shown in Table V., discloses but one type of reaction-time behavior under these conditions, namely, a type which is characterized by increased reaction times during deception. This type will be called the positive type, following the nomenclature we have been using. That it is truly a deception type is substantiated when the quantitative results and introspections are checked against each other. In general, the introspections are similar to those of the positive-type *O*s in Experiment I. That is, the positive-type *O*s in Experiment II., as in Experiment I., were conscious of deceiving and actually tried to deceive. This consciousness of deception caused strain, tension, and excitement, confusion, and at times unpleasant feeling and lack of ease when reversing the operations indicated; and it caused a feeling of relief and relaxation when obeying instructions. A few selections have been chosen from the introspections which will illustrate the above mentioned points.

TABLE V

Observer	Average "C" Reaction Times	Average "O" Reaction Times	Difference between "O" and "C" Averages	Probable Error of Difference	Probable Correctness of Difference
A	15.4587	17.0937	1.6350	.3018	1.0000
B	17.9965	18.4900	.4935	.3267	.8450
D	19.3675	19.4237	.0562	.3648	.5450
E	10.0425	11.0400	.9975	.1747	1.0000
H	10.1337	11.2437	1.1100	.1808	1.0000
L	12.8737	13.3737	.5000	.2637	.9000
Mc	12.9112	13.5487	.6375	.2718	.9400
M	16.6312	17.1462	.5150	.3042	.8750
R	16.9187	17.7500	.8312	.3251	.9600
S	8.5025	8.6137	.1112	.1310	.7175

[11]We were not, in this experiment, interested in the time required to make the choice between obeying and disobeying, but in the effect of this choice upon the subsequent operations.

[12]See Footnote 3.

A, 1. "I tried some real deceit."

E, 1. "When I reversed the operations there immediately came to consciousness a feeling as if I must put on the brakes and be more careful. I would decide to reverse the operations just when the examiner said 'ready'; then when ordered to 'go' there was a new decision to make other than I had expected. Confusion resulted."

E, 2. "The hesitation caused when reversing the operations seems to be due to the fact that a decision must be made for each problem. I have been trying in both operations to regulate the amount of time so as to increase deception, but there is a tendency to go fast on certain cards because of performing the operations as told."

H, 1. "I did not feel at ease, but became excited."

H, 2. "I still continued to be excited and only occasionally was I able to relax. I was conscious all the time that I was disobeying. I felt practically no relaxation in the experiment. It was more strain. When I was disobeying my voice quivered because I was not so sure of myself."

Mc, 1. "I had a greater feeling of strain when disobeying. I had an unpleasant feeling when disobeying but not when obeying. I had a feeling of relaxation when both the O and the C cards of each set were completed, but especially when the O cards were completed."

Mc, 2. "The unpleasantness and strain were greater when doing the O cards than when doing the C cards."

M, 1. "I felt a relief at the end of a card, more so when disobeying than when obeying. I had to force myself to work fast when disobeying, for there always seemed to be a little resistance coming forth which I had to overcome."

M, 3. "Disobeying caused confusion, I having forgotten just what order I am following.

R, 3. "In changing from obedience to disobedience, I find it hard to get started. I tried hard to deceive."

In the cases of D and S, it should be noted that the probable correctnesses of the differences between the "O" and the "C" reaction times were, respectively, .5450 and .7175, so low that a classification of them with the other *O*s as of the truly positive type is not warranted. When the introspections are checked up with the quantitative measurements, further proof is given for the fact that in these *O*s deception was not truly induced by the experiment. A few excerpts are taken from their qualitative data to validate the above statement.

D, 1. "There is no emotional difference in my consciousness when I obey or disobey instructions. My mind was as calm as it was possible to make it. I do not have any noticeable difficulty in reversing instructions. I did not try to hurry either when obeying or when disobeying, but allowed my mind to take as long a time as needed to give the answers. I made no attempt to slow down the process when I was going rather fast, whether I obeyed or disobeyed, and gave the answers just as they came. I tried to make myself as unconscious and indifferent to the time that was passing as possible, and simply to give the answers as they came without thought of trying to deceive the experimenter."

D, 2. "I decided to obey or disobey usually upon looking at the card. If the numbers were small, I reversed the signs; if large, I performed the operations as they stood."

D, 4. "I am quite indifferent to whether I obey or reverse and I do not seem to find one more difficult than the other."

TABLE VI

Observer	Number of sets on Day	Average "C" Reaction Times	Average "O" Reaction Times	Difference between "O" and "C" Averages	Probable Error of Difference	Probable Correctness of Difference
A	8	19.7600	21.5100	+1.7500	.4881	.9925
	8	15.5300	17.5100	+1.9800	.4699	.9975
	6	14.1600	13.9000	− .2600	.4649	.6500
	10	12.7400	15.1440	+2.4040	.3478	1.0000
B	8	19.2550	21.6450	+2.3900	.6425	.9925
	8	19.1600	19.4000	+ .2400	.8409	.5750
	8	16.6450	16.7250	+ .0800	.4565	.5475
	8	16.9250	16.1900	− .7350	.5682	.7750
D	5	26.6640	27.7360	+1.0720	.9491	.7750
	7	26.4785	26.1285	− .3500	.0946	.9925
	8	17.0500	17.7350	+ .6850	.5519	.8000
	6	16.2400	14.9333	−1.3067	.4034	.9850
	6	17.3866	17.5133	+ .1267	.4069	.9825
E	8	11.4550	13.1600	+1.7050	.2973	1.0000
	8	10.3800	11.8950	+1.5150	.3183	1.0000
	8	9.7500	10.4850	+ .7350	.2738	.9650
	8	8.5850	8.6200	+ .0350	.2044	.5475
H	8	11.5200	12.0450	+ .5250	.4140	.8000
	8	11.0450	12.8500	+1.8050	.4022	1.0000
	8	9.2050	10.6550	+1.4500	.3540	.9975
	8	8.7650	9.4750	+ .7100	.2393	.9750
L	4	16.7400	18.5300	+1.7900	1.0197	.8800
	7	16.9642	17.2428	+ .2786	.3216	.7200
	5	11.5360	11.4160	− .1200	.5895	.5625
	8	12.7550	13.6650	+ .9100	.4812	.9000
	8	11.2850	11.3600	+ .0750	.3278	.5625
Mc	6	18.7866	18.0200	− .7666	.4791	.8625
	6	12.4800	13.4866	+1.0066	.3459	.9750
	8	12.1600	13.6950	+1.5350	.3043	1.0000
	8	11.0250	11.5500	+ .5250	.3142	.8675
	4	10.0200	10.6400	+ .6200	.4447	.8250
M	4	22.1000	22.4500	+ .3500	.7125	.6375
	8	19.6050	19.5600	− .0450	.5140	.5300
	4	16.3900	16.7100	+ .3200	.3585	.7375
	8	14.5250	15.2600	+ .7350	.2637	.9700
	8	13.1500	14.1800	+1.0300	.3409	.9800
R	4	23.9500	24.9300	+ .9700	.9245	.7625
	7	24.5642	25.7500	+1.1858	.1309	1.0000
	5	15.2800	16.7280	+1.4480	.4644	.9825
	8	14.7400	14.9050	+ .1650	.3152	.6375
	8	14.2150	15.1300	+ .9150	.2643	.9900
S	6	10.6800	11.0266	+ .3466	.6811	.6875
	10	8.4880	8.4400	− .0460	.2517	.5625
	8	7.8950	7.9150	+ .0200	.1700	.5300
	8	7.4950	7.7200	+ .2250	.2164	.8500

The following introspections of S were obtained by further experimentation. It will be remembered that S has had considerable practice with the kind of work which constituted the material of both Experiment I. and Experiment II.

S. "The attitude is apparently the same toward each set. I am completely indifferent to the various sets. I am as willing to tell the truth as to tell a falsehood, especially as I gain nothing by lying. The difficulty of the two forms is about the same. The work has become mechanical. I am not aware of any difference between my attitude when obeying and my attitude when disobeying."

Examination of Table VI. shows that 7 of the 10 *O*s of Experiment II. had reversal days, that is, days on which the average of the O reaction times was less than the average of the C reaction times. E, H, and R had no reversal days. D and S had reversal days, but, as has already been noted, the nature of both their introspections and their quantitative measurements prohibits the conclusion that they actually practised a true deception during the experiment. In the case of the other 5 *O*s, A, B, Mc, M, and L, analysis of their introspections for the reversal days reveals that the attitude of these *O*s on these days was different from their attitude on normal days and that it was not the typical attitude of deception.

A, 3. "I notice quite a good deal more ease in computing the O cards than in computing the C cards."

B, 4. "It seemed to make no difference to me whether I disobeyed or obeyed. I usually obeyed with the smaller numbers and disobeyed with the larger ones. I tried to go as fast as I could in both cases."

L, 3. "There was really no difference in my consciousness in obeying or otherwise."

Mc, 1. "There was no feeling of strain or unpleasantness. The longer I did it, the easier it became to do both O and C, and the more pleasantness accompanied the experiment."

M, 2. "The usual hesitancy when disobeying was not so common as formerly." [13]

Conclusions.—In Experiment II., we attempted so to alter the procedure of the first experiment as actually to induce our *O*s to practise deception. The introspective reports bear testimony to the success of this attempt in the cases of 8 out of the 10 *O*s. The quantitative results for these 8 *O*s show that their reaction times were increased during deception, with relatively large probabilities that the increases were not due to chance. In the cases of the two remaining *O*s the conditions which were designed to induce deception (disobedience of the indicated operations) did, indeed, increase their reaction times, but the differences were so slight that they may have been due to chance factors. And these two *O*s reported, introspectively, no deception. Thus, reaction times were increased when disobeying in those cases in which the *O*s were conscious of deception.

[13] See Footnote 8.

This conclusion is supported by the fractionation of the data by days. The "O" reaction times were less than the "C" reaction times on those days on which the *O*s reported that they had little or no consciousness of deception.

We find, therefore, complete agreement between the two parts of the work. The situation in Experiment I. was only in small part successful in inducing deception. Whenever it did induce deception, the "O" reaction times were longer than the "C" reaction times; whenever it failed to do so, the opposite was true. The so-called positive and negative types of response were, therefore, not two types of response to deception, but a deceptive and a non-deceptive type of response to the particular experimental situation. When the experimental setting was changed in Experiment II. so as to render more effective the factors which tended to produce a consciousness of deception, all of the *O*s became positive in type, that is they tended to give the desired deceptive response. Two *O*s, in whom this tendency was not marked, failed to report deception. Five other *O*s were negative on certain (reversal) days, that is, they gave non-deceptive responses on these days, as evidenced by their reports. In both experiments consciousness of deception was accompanied by lengthened reaction times, as compared with the reaction times when the *O*s were not conscious of deception. The reaction times were in no case an indication of obedience or disobedience to instructions, but were, on the other hand, an indication of conscious deception.

Other investigations have shown that various so-called symptoms of deception, including reaction times, are not indications of objective lack of truth, but only of conscious deception. Leach and Washburn (7, p. 64) found several cases in which the reaction times to words concerning which the *S* was to practise deception were not longer than the reaction times to other words. But they also found that in the majority of these cases the factor of deception was inoperative. Marston (8, p. 162) came himself to the conclusion that "the behavior of the blood pressure does not act as the least indicator of the objective validity of the story told by any witness, but it constitutes a practically infallible test of the *consciousness of an attitude of deception*" (italics Marston's). Burtt (2) similarly recognized that the inspiration-expiration ratio is a symptom of the deceptive consciousness, rather than of objective truth or falsity. So, we have shown that the time required to perform simple computations is increased by a consciousness of deception, but is not increased by objective, but unconscious, falsity of the responses. If the results are applicable to the more heterogeneous conditions of association-reactions to words, increased association-reaction times may be taken to indicate conscious deception, but not objective falsity, as all investigators prior to Marston have found.[14]

[14]Marston (9) found that the mean variations in reaction times tended to show the same differences (positive or negative) as the reaction times themselves. Our results show no constant tendency of the mean variations in reaction time during deception. The data are, therefore, omitted here, but may be found in the manuscript copy referred to in Footnote 3.

Summary

1. The time required to perform simple exercises in addition and subtraction was increased by the consciousness of deception.

2. The consciousness of deception appeared as feeling of strain, self-consciousness, hesitation, conflict of impulses, and emotional disturbances.

3. A repetition of Marston's experiment showed that the experimental setting failed in large measure to produce deception, and that his so-called "negative" deception-type was, in reality, a type of non-deceptive response to the particular situation.

4. When the situation was changed so as to make the consciousness of deception occur more frequently, all observers became "positive" in type. In other words, they gave deceptive responses.

5. If the results obtained in simple computations may be generalized to apply to other situations, increased reaction time is a symptom of conscious deception, but not of objective falsity.

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A CASE OF SYNAESTHESIA

By Leon Ginsberg, Columbia University

A search of the literature on Synaesthesia (surprisingly large) fails to reveal any case in which secondary sensations of color appear upon the stimulation of each one of the sense organs. Colored hearing, colored odor, colored taste, or a combination of the three, is comparatively common. But no report has been published of any individual in whom every primary stimulus capable of producing a sensation is certain to cause the simultaneous appearance of a secondary color sensation.

The following report concerns itself with the synaesthesia of the author. He was first cognizant of the phenomenon during the year 1918. Whether it had been present before that year he is unable to say. During that year he was physically run-down and nervously instable. Whether he became subject to these secondary sensations because of his physical condition he is unable to assert.

Various Forms of Synaesthesia and Allied Phenomena

1. Colored hearing:
   - (*a*) Colored vowels;
   - (*b*) Colored consonants; the consonant taking the color of the vowel used in naming it;
   - (*c*) Colored music, colored tones, colored auditory imagery of tones;
   - (*d*) Colored noises.
2. Colored tastes.
3. Colored odors.
4. Colored cutaneous sensations:
   - (*a*) Colored temperature;
   - (*b*) Colored touch;
   - (*c*) Colored pain.
5. Colored kinaesthetic sensations.
6. Colored organic sensations.
7. Number forms, forms for the days of the week, months of the year, centuries.

The following color-labels are taken from the standard colored Chart for Elementary Color Instruction based on the Bradley Color Standards. The chart was used whenever and wherever possible, but the secondary color sensations are so varied, they change so slightly but definitely in shade and tint, that it was found impossible to find enough color variations on any chart available. For this reason, even where the colors of the chart are used, it must be understood that they are only approximate.

The Letters of the Alphabet

A(day): light, bright, beautiful apple green; yellow green, tint 1.
Ah(rah): dark brown, almost black.
Aw(raw): dark brown.
E(met): tan, color of a new pine-board.
E(me): white.
I(sight): strange blue; nearest equivalent is the flash of lightning
I(hit): dull silver or aluminum.
O(so): dark blue.
O(hot): almost black.
U(sounding like few): milky blue with some white in it; almost grey.
Ae(aesthetic): green.
Oo(good): dull grey.
Oi(void): blue green.
Ow(now): dark blue.

Taste

Sweet: orange red standard.
Salt: intensest blue; blue, shade 1.
Sour: intensest greenish blue; standard green.
Bitter: black.

Odor

Oil of Cloves: standard orange.
Rhubarb and Soda: yellow green, shade 2.
Camphor: standard red.
Peppermint: yellow, shade 1.
Iodine: yellow-green standard.

Temperature

Cold: white.
Normal: neutral dark grey.
Extreme cold or warm: reddish (color of pain).

Pain

Headache: black.
Intense headache: red.
Toothache: blue.
Intense toothache: red.

Tones (on Piano); Fourth Octave above Middle C

D: green, shade 1.
A: blue, tint 2.
D♯: blue-green, tint 2.

Third Octave above Middle C

D♯: green, shade 2.
F♯: blue-green standard.
C♯: green-blue standard.

First Octave above Middle C

A: green, shade 1.
B: yellow-green, shade 2.
G♯: green-blue, shade 2.

Middle Treble

C: red standard.
E: red, tint 1.
F: red-orange standard.

First Octave below Middle C

E: red, shade 2.
A: red, shade 1.
G: orange, shade 1.

Second Octave below Middle C

F: deep brown.
A: deep brown, a little lighter than the color for F.

Third Octave below Middle C

B: deep brown with a blue tinge, almost maroon.
E: black brown.

The highest notes run through a series of color, blue or grey to a silvery grey, and finally the highest notes of all become a dull silver. It will be seen, then, that the range of colors runs from a brown of a very dark shade, almost a black, for the lower notes, those of the third octave below middle C, through a deep brown for the higher notes, of the second octave below middle C, through orange-red and red for the notes of the first octave below middle C, through red for the notes of the middle octave, through green-blue and yellow-green, blue-green and green and green-blue for the notes of the third octave above middle C, and through green-blue for the notes of the fourth octave above middle C.

The colors therefore run as follows:

Black Brown
Brown
Dark Red
Orange Red
Lighter Red
Blue Green
Green Blue
Cold Blue
Dull Grey
Silvery Grey
Dull Silver

The above list shows not a very close, if any, resemblance to the spectrum. As the tones run through a series of dark, heavy, massive bass notes to notes gradually lighter, less massive and more tinkling, so too the colors run through a series from dark, heavy colors, to lighter and lighter colors, until grey is reached.

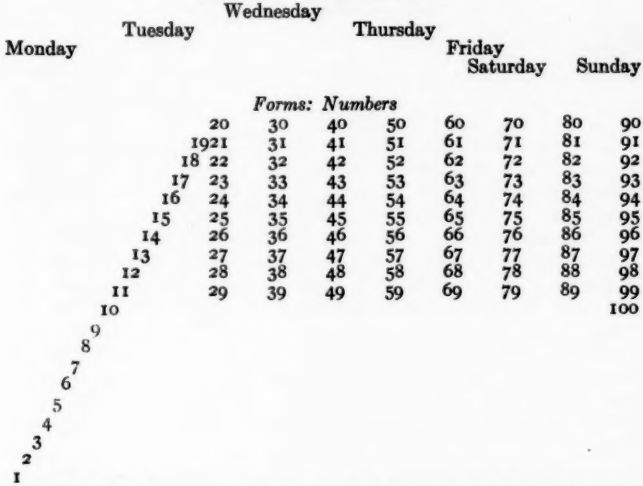
Photisms for Digits

The digits take on the same color as the vowel used in saying the digit. Thus:

Digit	Vowel Component	Vowel Color	Digit
1	*u* (hut)	dark, dull blue	dark, dull blue
2	*u* (oo)	blue grey	blue grey
3	*ee*	white	white
4	*aw*	dark brown	dark brown
5	*i* (fight)	metallic blue	metallic blue
6	*i* (fit)	dull silver	dull silver
7	*e* (met)	tan	tan
8	*a* (ate)	green	green
9	*i* (fight)	metallic blue	metallic blue
10	*e* (met)	tan	tan

The days of the week take on the color of the vowels used in saying each day. Thus Monday has two concomitant colors, dark bluish grey and green.

Forms: Days of the Week



Forms: Numbers

The following experiment was performed to determine the permanence of the secondary sensations.

	July 1921	Nov. 1921
	Middle Treble	
C:	red standard.	O. R. standard.
E:	red, tint 1.	Red, tint 1.
F:	red-orange standard.	Red-orange standard.
	First Octave above Middle C	
A:	green-blue, shade 1.	Violet-blue, shade 1.
B:	yellow-green, shade 2.	Yellow-green, shade 2.
G♯:	green-blue, shade 2.	Green-blue, shade 2.
	Third Octave above Middle C	
D♯:	green, shade 2.	Green, shade 2.
F♯:	blue-green standard.	Blue-green standard.
C♯:	green-blue standard.	Blue, tint 1.
	Fourth Octave above Middle C	
D:	green, shade 1.	G. standard.
A:	blue, tint 2.	Blue, tint 2.
D♯:	blue-green, tint 2.	Blue-green, tint 2.
	First Octave below Middle C	
D:	red, shade 2.	Red, shade 1.
A:	red, shade 1.	Red, shade 1.
C:	orange, shade 1.	Orange, shade 1.

Second Octave below Middle C	
F: deep brown.	Deep brown.
A: deep brown.	Dark brown.
C: deep brown, almost black.	Blackish brown.
Third Octave below Middle C	
B: deep brown, with blue tinge; almost maroon.	Dark purple.
G: black, with brown tinge.	Black.
E: black-brown.	Dark brown.

After a five-month period the secondary sensations present practically no change. What slight variation appears, is one of shade and tint rather than color.

Many authors have reported cases of fusion of the secondary sensations in accordance with the laws of color mixture. Other authors have failed to find any such fusion. The following experiment was performed. The same piano-notes as above were sounded in pairs as chords, five months after the colors for the individual notes had been determined. There was no fusion of the secondary sensations. As the chord was sounded, the colors for the notes composing the chord were seen to run like two colored ribbons side by side. Repeated sounding of the same chord made the color of each note appear more clear, but at no time was there fusion. An effort was then made to see whether the color of each note of the chord was the same as the color determined for it five months previously, when it was sounded alone.

Color of Note Sounded Alone	*Color of Note Sounded in Chord*
Middle Treble	
C: red standard.	Orange-red standard.
E: red, tint 1.	Green-blue, shade 1.
First Octave above Middle C	
E: yellow-green, shade 2.	Blue standard.
G♯: green-blue, shade 2.	Orange-red standard.
Third Octave above Middle C	
D♯: green, shade 2.	Red, tint 1.
F♯: blue-green standard.	Violet-red, tint 1.
Fourth Octave above Middle C	
D: green, shade 1.	Green-blue, shade 2.
A: blue, tint 2.	Blue, tint 2.
First Octave below Middle C	
D: red, shade 2.	Red, shade 2.
A: red, shade 1.	Violet-red, shade 2.

The color for every tone, when sounded in conjunction with another tone, is found to be different from the color of the same tone when sounded alone. There seems to be a reciprocal reaction of one tone upon the other, with a concommitant change in the secondary sensations.

An attempt was made to see if any fusion of colors, comparable with laws of color mixture, occurred when taste was the primary sensation. In a glass of water at room temperature

(68°) three teaspoons of sugar were dissolved. The sugar was allowed to dissolve thoroughly, and was distributed as evenly as possible through the water by stirring. The water was sipped, and the color of the secondary sensation noted. A glass with an equal volume of water at the same temperature, in which was dissolved a teaspoon of table salt, was sipped (on the next day), and the color of the secondary sensation was noted.

Into a bottle was now poured a glass of water containing dissolved in it three teaspoons of sugar, and a glass of water of equal volume into which had been put a teaspoon of salt. The water was thoroughly stirred. The temperature was as before. The mixture was sipped, and the color of the secondary sensation was noted. The results were as follows.

Sweet: R. O. standard.
Salt: blue standard.
Mixture: violet-red, tint 1.

There is a fusion, and the color obtained is an intermediate between the colors for salt and sweet, red-orange and blue.

Fusions of sensations from different senses were next tried. The tongue was projected from the mouth, the mouth was shut, and the nose was plugged with cotton. Oil of cloves was put upon the tongue. The color of the secondary sensation was recorded. The mouth was then tightly closed and the secondary sensation of the odor of oil of cloves was recorded. Then the tongue was once more projected, oil of cloves was put on it, and at the same time the bottle of oil of cloves was put under the nose. The secondary sensation was noted. The same experiment was tried with rhubarb and soda.

Oil of Cloves

Taste: blue, shade 2, almost black.
Odor: standard orange.
Taste and Odor: red, shade 2.

Rhubarb and Soda

Taste: orange-red standard.
Odor: yellow-green, shade 2.
Taste and Odor: dark green (yellow green, shade 2).

Here too, there was fusion, and once more an intermediate color was obtained.

Localisation

Colors are projected in the direction of the sound, if the sound comes from without. In inner speech, as when the letter *a* is inwardly pronounced, the color is located somewhere within the head; and so rapidly does the color follow upon the sound that it is located, I am almost sure, just against that part of the palate and mouth used in pronouncing a vowel. Thus, the white

photism for the vowel *e* (me) is localized against the roof of the mouth, and the photism for *e* (met) is localized down in the throat.

Taste photisms are localized in the mouth, and odor photisms in the nose. Temperature photisms are localized on that part of the body experiencing the sensation. Pain photisms are localized on the part of the body experiencing the pain. The photisms for all the sensations are localized on the part of the body concerned in the primary sensation.

The term 'synaesthesia' has been used so loosely that it has led to confusion. The two leading theories, the association theory and the physiological theory, have adherents, just because of this blanket-term for two groups of different phenomena. All adherents of the association theory present proof which points clearly to an associational cause. And the adherents of the physiological theory present very pertinent material to uphold a physiological causation. Both theories are correct. But each, more or less adequately, accounts for phenomena entirely different. The phenomena should not be included under the same class-name. Due to association are types of colored hearing in which a color is attached to an object for manifestly associative reasons, number forms, forms for days and months, personifications of various sorts, and association between colors and theories (of history, etc.) and colors and abstract terms (there is present here, I believe, a common affective tone serving as an associative bond). Under pure synaesthesia should be included colored hearing, colored taste and odor, colored temperature, pain, etc., gustatory audition, etc. Number forms, etc., and colored hearing are very often found together, but this conjunction does not mean they have a common casual factor. For just as many individuals have number forms and various personifications, etc., and yet betray no evidence, at the same time, of colored hearing.

To say that synaesthesia is due to associations formed in childhood seems to me not to be in accord with the facts. There is too great a dependence of the secondary sensation upon the primary sensation. The slightest change in the primary sensation causes a proportional change in the secondary sensation. There are as many concomitant color changes of shade and tint as there are possible changes in the primary sensations. The number of colors which are definitely 'seen' as photisms are too varied, too definite and permanent, to have been learned in childhood, or indeed in a life-time. The color of a new odor or taste combination (a mixture of oil of cloves and rhubarb and soda never before tasted) gives as definite and immediate a photism as does a primary sensation many times experienced. Associations so ingrained, so definite and fixed, as are the color photisms

would remain so through life (especially if formed in childhood). Yet synaesthesia usually disappears in senescence, and indeed very often after adolescence.

It seems to the author that synaesthesia is not a phenomenon peculiar to about 15% of the population, but is, in some form or other, true of everyone. It would be better to say that in 15% of the population it is so intense as to be more noticeable than in the other 85%. Literature gives a clue to the universality of the secondary sensations. Most, if not all of us, seem to agree with the following descriptive phrases; the silvery tone of the violin, the red blare of the cornet, the golden voice of the tenor, etc.

Titchener says (*Text Book of Psy.*, 194 ff.): "Every sensory stimulus of moderate intensity arouses a wide-spread reaction. It has been shown, for instance, that the sounding of a single note upon the harmonium will not only bring about an adjustment of the organs of hearing, but will also call out visual, verbal and other associations. . . .

"Sometimes they [the phenomena of synaesthesia] seem to depend upon a purely individual feature of nervous constitution, as when a sour taste makes the scalp itch; sometimes they are common to a large number of persons: most of us shudder and grit the teeth when we hear the squeak of chalk against the blackboard, or the grating sound of a saw against metal. The connection may obtain between separate sense-departments, as sound and sight, or between areas of a single sense-department; thus an itching of the nose often comes along with pricking sensations at the back of the neck on the same side . . . In some cases, the concomitance is stable, in others it is highly variable; in some it is limited, as it were incidental, in others systematic, extending to an entire series of qualities."

SOME OBSERVATIONS AND EXPERIMENTS OF THE INTELLIGENCE OF THE CHIMPANZEE AND OURANG

By Wm. Thomas Shepherd, Columbia University School

The experiments and observations reported herein were made on two apes, Sokker, a male chimpanzee 7 years old, and Rufus, a male ourang 4 years old, in the National Zoological Gardens, Washington, D. C.

Purposes: I. To test their mentality for (1) ideas, ideation, (2) reasoning powers, and (3) imitation as involving ideation.

II. To test them by experiments similar to experiments made by the present writer on Rhesus monkeys some years ago; to ascertain if their intelligence is equal to, or greater than, that of other monkeys, so far as these tests would indicate.

III. To verify conclusions reached previously in tests on other apes (the trained vaudeville stars Peter and Consul) observed and previously reported on by the present writer.

IV. To test their intelligence as compared with that of dogs and cats in similar experiments made by the writer at the George Washington University.

Tests of Reasoning

In this test, as in those on Rhesus monkeys, dogs, and cats, referred to above, I suspended a piece of apple (for which the apes have a liking) by a string in front of the cage beyond their reach; it had a piece of stick run through it, one end of which was pointed toward the cage in which the animals were kept; that end of the stick was within the reach of the apes if they would pass their hands through the wire of the cage. The problem was to see if they would reach out and seize the end of the stick, and draw in and thereby secure the piece of apple.

Sokker, the chimpanzee, reached through the wire, seized the end of the stick, and drew in the piece of apple: in the first trial, in 5 sec.; in the second trial on that day in 4 sec. Rufus, the ourang, succeeded in securing the piece of apple in the same manner: in his first trial, in 8 sec.; in his second trial on that day in 6 sec.

These experiments were repeated the next day. Both animals did the act, and in shorter time. In this experiment both gave what I consider to be clear indications of a low form of reasoning,—what I have called, in the previously reported experiments on Rhesus monkeys, Adaptive Intelligence.

Tests of Ideation

To test the animals I employed a plan formerly used with Rhesus monkeys. I placed a light board, 30 in. long and 8 in. wide, outside the cage and at right angles to it, upon an improvised platform on a level with the floor of the cage. One end of the board was within reach of the animals through the wire of the cage; the other end was beyond their reach; a piece of banana, for which the animals have a fondness, was placed out of reach on the board. The problem was to see if the apes would take hold of the end within reach, pull the board in, and thus secure the banana.

Both the animals did the act successfully. The figures are: Sokker, first trial, 20 sec.; second trial, 15 sec. Rufus in the first trial took 25 sec.; in the second trial 16 sec. We might remark that Rufus appeared somewhat fearful and cautious. The next day the experiments were repeated. Sokker secured the banana in the first trial in 8 sec., and in the second trial in 6 sec. Rufus took 9 and 6 sec. respectively in his first and second trial on that day.

Tests of Imitation

In these tests I employed a method used by Hobhouse in England on a chimpanzee and Cebus monkey, and by Watson at Johns Hopkins Uni-

versity on monkeys. I had employed the plan in tests on Rhesus monkeys, reported in Monograph Supplement No. 52 of the Psychological Review. In these tests on the monkeys, I had secured only negative and partially positive results.

A T-rake was used; food, a piece of banana, was placed in front of the cage and in the apes' sight but beyond their reach; the T-rake was placed with the end of the handle within their reach; the problem was to see if they would take hold of the handle and draw in the food, after seeing me do it; if they would imitate my actions. I demonstrated the use of the rake three times, and then gave each separately (they were in different cages) a chance to imitate my act, and thereby to secure the piece of banana.

Sokker in the first trial immediately seized the end of the rake handle and pulled it under the lower part of the wire into the cage; he examined it carefully, and played with it; but he made no effort to use the rake to draw in the food as I had shown him how to do. The banana was, of course, within the sweep of the rake, if he had used it. He was given 2 min. to get his ideas together, if he had any, in the matter. In his second trial, he repeated the actions of the first trial; he made no attempt, so far as I could observe, to use the rake to secure the banana. He at times looked, or appeared to look, at the banana. In similar tests next day, two trials under similar conditions and time, he entirely failed.

Rufus, on the first day, first trial, was allowed 2 min. He repeated essentially the actions of Sokker. He seemed, perhaps, to be more fearful and cautious. He failed likewise in the second trial.

Consideration of Results

A comparison of the results of the experiments on reasoning (of the low form) shows that the apes did the acts the Rhesus monkeys did, and in quicker time. The experiments with the apes were fewer in number, but seem to me to be significant. Their mentality, as compared with that of the Rhesus monkeys, seems to be superior, so far as these experiments show. The dogs and cats tested failed entirely or almost entirely in similar experiments. But we should bear in mind that the latter have not the motor equipment, that is, the hands, that the apes and other monkeys possess. In every way, however, they seem to be inferior in intelligence.

In the tests on ideation, in general the apes surpassed the Rhesus monkeys, and by far the dogs and cats tested. That is, they did all, or nearly all the acts the monkeys did, and in shorter time; their understanding of a given situation, like these here reported, was better.

In the test for imitation here reported, two of the Rhesus monkeys did make a success, or at least a partial success, in similar trials. But they were allowed many more trials than the apes. They did not succeed in the number of trials given the apes. For myself, I am not convinced that the apes under similar conditions of more trials would not have done as well as, or better than, the Rhesus monkeys, supposed biologically to be their inferiors. I suggest that they probably would have done so.

Summary

I. The superior motor equipment of the apes accounts in large measure for their apparently superior intelligence.

II. Their semi-erect and biped position accounts in part for their alleged superior intelligence.

III. (1) They give indications of ideas, probably of a crude and unanalysed type. (2) They give evidence of a form of intelligence which may roughly be called a low form of reasoning—as is shown by the suspended-apple experiments here reported.

IV. Observations and experiments, as well as the theory of evolution, appear to give the apes, at least the chimpanzee and ourang, a place above all other sub-humans, in intelligence, as in anatomical structure.

The writer is fully aware that the experiments here reported are few in number. But they suffice, taken together with other observations, to make him reasonably confident of these conclusions.

CONCERNING QUANTITATIVE PLETHYSMOGRAPHIC TECHNIQUE

By SAMUEL W. FERNBERGER, University of Pennsylvania

Several years ago Hartman and McDonough[1] reported a method for making quantitative records of the increase and decrease in the blood volume of the forearm. The method consists in inserting a graduated glass tube for the usual glass tube of a Lehmann plethysmograph, and thus being enabled to read the volumetric changes directly. If the volume of the entire plethysmograph is known, and if the amount of water inserted is known, one can then ascertain the volume of the arm involved in each observation. It is thus possible to express the results in terms of percentage of increase or decrease of the volume of the arm involved. These authors used diastolic blood pressure to determine the maximum expansion or elasticity of the vascular system. In this part of their experiment, a Stanton Blood Pressure Apparatus was applied to the upper arm while the same forearm was inserted in the plethysmograph. The bag was then inflated until the veins were closed while the arteries remained open. Thus the blood continued to flow into the forearm without any possibility of flowing out, and so the vascular system was stretched to the limit of its elasticity. Hartman and McDonough also determined the percentage of increase and decrease for a purely physiological function, such as holding the breath, and for mental work, such as difficult mental multiplications. These investigators report one observation each for 24 subjects. In general they found the following results. A considerable variation was found from *S* to *S* in the increase due to diastolic pressure. The average for all *S*s was 1.76 *per cent.* of the volume of the arm. Much smaller variations were found for holding the breath and for mental work, and the peripheral changes due to these tasks never approached the limits of elasticity.

It seemed worth while to repeat this experiment, taking a number of observations at intervals from each of a group of *O*s, to ascertain the variability of a given *O* from time to time. The present study reports such observations on 10 *O*s. The number of observations ranges from 6 to 20 per *O*, taken at intervals of a week. Diastolic pressure, holding the breath and mental work were again used, so that the results might be directly comparable with those of Hartman and McDonough.

In a preliminary series of experiments it was ascertained that the column of water in the graduated tube almost invariably came to rest, after an observation, at a point lower than when the observation had been started.

It was believed that the cooling of the water in the plethysmograph, and its accompanying contraction, might be the cause of this lowering. Hence we inserted a thermometer through the metal tube, so that the bulb was entirely immersed in the water over the wrist. The metal sleeve was covered with cotton-wool and had an outer covering of oil-cloth. It was discovered that water inserted at 98° F. cooled less than 2° F. during a 15 min. observation. This cooling is not great enough to account for the disparity of the readings before and after an observation.

It was then noticed that the elbow had a tendency to slip upward in the elbow-rest during a long observation. The usual padded arm-rest had been used. We then discarded this type of arm-rest, and made a plaster cast of the elbow for each *O*. With the plaster-cast elbow-rest the *O* had little trouble in keeping the arm in position, and this source of error was entirely eliminated.

[1] G. N. Hartman and D. L. McDonough, On Arterial Expansion, this JOURNAL, 32, 1921, 516–518.

Obs.	Number of Observations	% of Change Diastolic	P. E.	% of Change Holding Breath	P. E.	% of Change Mental Work	P. E.
1	11	+3.01	0.222	−0.37	0.046	+0.04	0.046
2	13	+1.51	0.090	−0.02	0.047	+0.15	0.036
3	18	+1.66	0.425	+0.33	0.039	−0.39	0.072
4	19	+1.82	0.115	+0.44	0.038	+0.03	0.032
5	20	+1.98	0.093	+0.48	0.078	−0.22	0.025
6	20	+2.21	0.115	+0.34	0.052	−0.07	0.252
7	6	+1.80	0.181	+0.43	0.044	−0.10	0.049
8	18	+1.29	0.114	+0.24	0.038	−0.01	0.036
9	6	+2.64	0.126	+0.22	0.074	+0.17	0.044
10	18	+1.40	0.069	+0.16	0.024	+0.11	0.024

The results reported in the accompanying Table were all taken with plaster-cast elbow-rests. The *Os* and the number of observations for each are given in the first two columns of the table respectively. In the third column are given the averages for the percentage of change under diastolic pressure, with the probable errors in the fourth column. In the fifth and sixth columns are given the average percentage of change and the probable errors for holding the breath, and in the final two columns the corresponding values for mental work. In all cases the averages were calculated taking account of the sign of each observation.

Our results agree exactly with those of Hartman and McDonough. The increase for diastolic pressure is very much greater than for either of the other conditions for all of our *Os*. The general average is 1.93 *per cent.* The probable errors are larger than one would desire but, considering the inaccuracy of the present best methods of measuring blood pressure, they are probably as small as one could expect.

For holding the breath the averages show an increase for 8 *Os* and a decrease for 2. The percentage of change is not great, however, and the probable errors are so large as relatively to invalidate the results in most cases. This is because no *O* gave all of his determinations either as increases or as decreases of volume. These results are somewhat at variance with those of Hartman and McDonough, who found that 16 of their 24 *Ss* showed a decrease, 5 showed no change, and only 3 showed an increase. We both agree that the reactions of the different *Os*, as well as of the same *O* from time to time, seem to be variable.

There *is* even greater variation from *O* to *O* in our results for mental work. Five show an increase and five a decrease. Again no *O* had all the observations in the same direction, so that the probable errors are so large as to lend little validity to the results. For mental work our results agree more closely with those of Hartman and McDonough. Of their 24 *Ss*, 13 showed a decrease, 4 no change, and 7 an increase.

We took account of temperature, weather conditions, blood pressure and a subjective estimate of the *Os*' general health and well-being; but none of these factors seems to correlate with the variations in the results.

It would seem that this extreme variability makes questionable the entire method of plethysmographic research in psychology. Apparently there are a number of factors, both physiological and mental, which may affect the plethysmographic results. For example, we are convinced that, during mental work, the trend of increase or decrease of the volume of the forearm is largely determined by the *O*'s breathing during the observation. The fact that the *O* may at times hold his breath during mental work we believe has a much greater effect on the plethysmographic curve than the actual work itself. Hence it may well be that we have been studying artifacts or indirect effects in our vascular work in psychology rather than the direct effects of our stimuli. This, then, may be the cause of the great

variability of plethysmographic results which Leschke[2] discovered when he attempted to statisticize the results of former investigators. However, this theory needs further investigation and research.

Summary

1. Continued observations on the same *O*s yield results, in quantitative plethysmographic research, that are very similar to those of Hartman and McDonough.

2. For the limits of elasticity, the probable errors are as reasonably small as one might expect with the present inadequate methods of taking blood pressure.

3. For holding the breath and for mental work, the probable errors are so large as probably to invalidate the averages.

4. The cooling of the water in the plethysmograph is so slight that it may probably be disregarded for a 15 min. observation.

5. The old-style padded elbow-rest of the Lehmann plethysmograph was found to be a source of considerable error which may be eliminated by substituting a plaster cast of the elbow for each *O*.

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[2]E. Leschke, Die körperlichen Begleiterscheinungen seelischer Vorgänge, *Arch. f. d. ges. Psychol.*, 21, 1911, 435ff.

GOERENZ' *VESTIGIA*

By E. B. Titchener

In his famous Note on the history of the association of ideas,[1] Hamilton refers to four earlier works: the *Geschichte* of Hissmann, the *Paralipomena* and *Beyträge* of Maass, and the *Vestigia* of Goerenz. The full titles, etc., of these works are as follows.

(1) M. Hissmann, *Geschichte der Lehre von der Association der Ideen, nebst einem Anhange vom Unterschied unter associirten und zusammengesetzten Begriffen, und den Ideenreyhen*, Göttingen, 1777, pp. [24 +] 144.

The date is given correctly by Hamilton, Maass (*Einbildungskraft*, 1797, 402), W. Volkmann von Volkmar (*Lehrbuch*, ii., 1885, 549), W. H. Burnham (this Journal, ii., 1889, 618), M. Dessoir (*Geschichte*, i., 1897, 211), etc.; it is given as 1776 by L. H. Jakob (*Grundriss d. Erfahrungs-Seelenlehre*, 1810, 118) and by Goerenz; and as 1772 by Dessoir (i., 1894, 307) in a Note which contains other errors.

(2) J. G. E. Maass, *Paralipomena ad historiam de associatione idearum*, 1787. *Dissert. inaugur.*

I have not seen this tract, which was written as a supplement to Hissmann's *Geschichte*.

(3) J. G. E. Maass, *Beyträge zur Geschichte der Lehre von der Vergesellschaftung der Vorstellungen:* Part iii. of *Versuch über die Einbildungskraft*, Halle & Leipzig, 1792.

I have not seen this first edition. In the edition of 1797 the *Beyträge* occupy pp. 311–453. It is reasonable to think that Maass has here repeated the substance of his *Paralipomena*,—which he does not mention; though Hamilton's reference to both works may be significant.

(4) J. A. Goerenz, *Vestigia doctrinae de associatione quam vocant idearum libris veterum impressa*, Vitebergae (Halle), 1791.

This dissertation seems to be extraordinarily rare. Maass does not mention it, although he brings his list of authors down to 1796; and Dessoir says nothing of it. I have been unable, by the ordinary means of search, to find it in London, Oxford, Edinburgh, Paris, Leipzig or Halle. I have finally run down a copy in the *Stadtsbibliothek* at Berlin, and have had a transcript made, from which I have prepared the following summary,—a small contribution to the archaeology, if not to the history, of psychology.

Goerenz sets himself a simple task of compilation, that of bringing together all the passages from ancient writers that anticipate the modern doctrine of association of ideas. He sees that there are two main difficulties in the way of a properly historical treatment: the lack of documents for the earlier period, and the danger of reading into his authorities more than they themselves intend. He avoids these difficulties, again very simply, by quoting his sources for what they are worth, and by cautious phrasing of his interpretations. His arrangement is chronological.

The first name on his list is that of Pythagoras (c. B. C. 580–500). If we may trust what we are told of Pythagoras' rules for the cultivation of memory, their author had some inkling, however vague and indefinite, of the two laws of succession (*ordo*) and resemblance. Simonides of Ceos (c. B. C. 556–467) invented a mnemonic art, which evidently implies the three laws of resemblance, simultaneity and succession (*ordo*); Simonides

[1] Sir W. Hamilton, *The Works of Thomas Reid, D.D., Now Fully Collected, with Selections from his Unpublished Letters*, Note D.**, *Contribution towards a History of the Doctrine of Mental Suggestion or Association* (ii., 1863, 889 ff.).

himself lays especial emphasis upon succession. We are still far from an express formulation of the laws of association; but Simonides' practice is surprisingly modern. There is no reason to think that the sophist Hippias (a contemporary of Socrates), or Theodectes (c. B. C. 376–335), or Metrodorus of Scepsis (of the time of Aristarchus) added anything of importance to his teaching.

Goerenz now makes his sole reference to American psychology; I quote in fuil. "I have this one thing to add," he says, "that the barbarous tribes of America make use of a certain art of memory which is not so very different from that employed in antiquity. For just as the ancients bound things fast (so to say) to certain places, by way of images, so do those barbarians connect things with cords from which are hung an immense number of little shells of various colors, and which they call 'belt' or 'string' or 'fathom of wampum.' By means of such cords, then, to which they take recourse on every occasion that is at all serious, and which they religiously preserve, they both strengthen and prolong their memory of past deeds; for when they look at any particular cord they readily recall what was said and done at the time when they first received it."[2]

From Simonides we take a long step forward to Plato (B.C. 427–347); Socrates does not come into our story. Goerenz discusses four principal passages: the experiment with the slave-boy, in the Meno; the proof of immortality from the doctrine of recollection, in the Phaedo; the image of the waxen tablet (origin of truth and error), in the Theaetetus; and the comparison of the mind to an aviary, also in the Theaetetus.[3] He concludes that Plato knew a good deal about the laws of succession (*ordo*) and resemblance, and had at least a vague and partial knowledge of the law of coexistence. Plato, moreover, is the first and perhaps the only ancient writer to derive and explain from these laws of association the origin and causes of false opinion. And so (the sentence is surely worth its space!) "nihil obstitisse Nostro videtur, quominus eo usque penetraret, quo nostra aetas hac in re evasit, nisi illud, paene dixerim vagum disputandi genus, quod sibi indulsit; quo etiam fit, ut saepius rem dimittat, antequam ab omni parte considerata excussaque fuerit."[4]

After Plato comes Aristotle (B.C. 384–322), whose teaching is summarised under eight heads. "(1) We recollect, when the affections from things perceived by the senses so follow one another in the mind that each one arouses the next. (2) When we recollect, those things which originally affected the mind first are first aroused, and so on until we have arrived at the thing which we wish to recollect. (3) In recollecting, we start out from the like, the contrary and the contiguous. For the affections which present themselves are either the same as formerly, or simultaneous, or include a part in such wise that the remainder can readily be recalled. (4) In recollection the affections come to the mind afresh in the same sequence in which they originally impressed it. Hence we readily recollect things which, when we learned them, were held together by the bond of succession (*ordo*); otherwise we recollect with difficulty. (5) Recollection in general should start out from some fixed first term (*principium*), namely, from the term from which everything else may be found most readily. This is the term that holds the middle place. Suppose, for instance, that the letters *abcefg* stand for certain ideas of things, and suppose that you have found by memory the middle letter *d*, and that this is not itself the letter you are in

[2]Goerenz' source is G. H. Loskiel, *Geschichte der Mission der evangelischen Brüder unter den Indianern in Nordamerika*, [1788,] 34 f. There is an English edition of 1794.

[3]See B. Jowett, *The Dialogues of Plato*, 1892, ii., 40 ff., 213 ff.; iv., 258 f., 262 ff.

[4]I must add that Goerenz goes on: "At iudicamus forte audacius?" But the question is merely rhetorical.

search of: you may then turn to *e* or *f* or *g*; and if the thing you are in search of is not among these, by retracing your steps you will readily discover it in *c* or *b* or *a*. (6) Sometimes we cannot recollect; and this happens if, in remembering, we wander off from some first term in all directions. (7) Sometimes, in recollecting, we are carried away in the direction of our dominant habit; here as elsewhere habit is a sort of second nature. (8) But most of all we are led astray, in recollecting, in the emotions and passions of the mind, as in fear or anger."[5] Aristotle thus gives us, explicitly, the four laws of succession (*ordo*), resemblance, opposition and simultaneity.[6] He also makes use of the doctrine of association for the explanation of dreams.

Goerenz is surprised and disappointed that Aristotle, having said so much, does not in other contexts say more; but we have seen that Goerenz does not shine as critic. He finds little to his point in subsequent Greek philosophy. Carneades (c. B. C. 160) seems to have had a general notion of the association of ideas; the Stoics do not touch it at all.

Among Latin writers, Quintilian (c. A. D. 40–118) clearly recognizes the laws of succession (*ordo*), resemblance and coexistence, in their application to the art of rhetoric.

Maximus of Tyre, a Greek writer of the age of the Antonines, next calls for mention. Maximus does due justice to the great law of succession (*ordo*), but for the rest occupies himself with sub-laws—of time (night follows day, winter summer), of affection of the mind (love follows beauty, anger insult), of place, of (logical) distribution, of meaning (*potentia*)[7]—which may evidently be multiplied at will, and which are in any event nothing more than special cases of the true laws. Maximus derives in part directly from Plato and in part, perhaps, from Xenocrates; hardly from Aristotle.

The Neoplatonists yield us nothing; the few remarks of Porphyry (c. A. D. 230–300) are trivial. We thus come to the Fathers. Tertullian (A. D. 165–220), though he wrote a book on the mind, says nothing of the association of ideas. Everywhere but in Augustine (A. D. 354–430) we draw blank: Augustine appears to have been the first to see "that all our experience is born of the law of resemblance." In general, he follows Plato.

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[5]Cf. W. A. Hammond, *Aristotle's Psychology*, 1902, 203–212. Goerenz' summary is preceded by a polemic against Maass' interpretation of the term κίνησις, which Goerenz translates, in psychological context, preferably by *motus* (in the metaphorical sense), though he will accept *affectio*. The summary is followed by a discussion of the questions whether Aristotle placed the *notionum perceptarum simulacra* in the brain, and whether he regarded them as in any way corporeal. Goerenz answers both questions in the negative.

[6]Warren has recently stated that I agree with Hamilton's rendering of the key-passage in the tract On Memory and Recollection. The statement is not quite correct. I should translate: "This, too, is why we hunt through the series in thought, starting from our present topic or from something else,—I mean, from something similar or contrary or from the contiguous;" I do not see that the καί can otherwise make sense. See H. C. Warren, *A History of the Association Psychology*, 1921, 25 ff.; W. Hamilton, *The Works of Thomas Reid*, Note D.** (ii., 1863, 897 ff.). Cf. W. Wundt, *Kleine Schriften*, i., 1910, 560 f.; ii., 1911, 182 f.; iii., 1921, 502 ff.; *Leibniz*, 1917, 38 f.; and cf. H. Siebeck, *Geschichte der Psychologie*, i., 2, 1884, 77. Siebeck translates τὸ σύνεγγυς by *zeitliche Aufeinanderfolge;* if that translation stands there can, of course, be only three Aristotelian 'laws.'

[7]Cf. Siebeck, *op. cit.*, 309.

Here Goerenz ends. "The ancients were undoubtedly aware that nothing occurs at haphazard in the association of our ideas, that the one always depends upon the other, that all are held together by a common bond. They knew that the first idea is joined to the second, the second to the third, either according to the order of succession that united them when they entered the mind, or on account of their resemblance to one another, or again on account of their dissimilarity, or lastly if they had formerly presented themselves to the mind at the same time. They saw too that here is the source of all the knowledge that we gather from experience; they warned of the power of association in preconceived opinions; they hinted that the emotions are governed by the same force; but they were content to show all these things as if from afar off." On the whole, we can excuse them: for Plato speaks of association only by the way, when he is occupied with something else; and the rest were interested merely to apply so much as they understood of its laws to the matter immediately in hand. All but Aristotle! from him, surely, we had the right to expect better things.—Alas, poor Aristotle!

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF NORTHWESTERN UNIVERSITY

Contributed by ROBERT H. GAULT

I. SOME DATA ON THE INTELLIGENCE OF JAPANESE CHILDREN

By TONAN FUKUDA

The following study of the intelligence of the Japanese children in Denver, Colorado, was made in June, 1922. The English version of the Stanford Revised Binet-Simon Intelligence Test was employed. However, in view of the double standard in their use of language, *viz.*, Japanese at home and English outside, it seemed wise to avoid the use of the tests which require familiarity with English, such as the Vocabulary Test or Repeat Syllable Test. Therefore, whenever possible, suitable alternative tests were used instead of these. Although a complete elimination was not possible, the disadvantage was removed to a considerable extent by this change.

The number of children tested is 43, which comprised practically all the Japanese children within our reach. They are classified according to their chronological ages and sex as follows:

Age	Number
Over 3	3
4	9
5	7
6	7
7	6
8	6
9	1
10	2
11	1
12	1

Sex	
Male	21
Female	22

The parents of these children are immigrants: none of them has been naturalized. Their home language is usually Japanese. They may be classified according to their occupations as follows:

Professional, intellectual work; large business	33
Small business; skilled labor	39
Unskilled labor	28

Almost all the children were neatly dressed and in good health, except one who was weak from digestive disturbance,—who, nevertheless, scored the highest point of all, *viz.*, I.Q. 143. None of these children was suffering from physical defect. In general they were in good humor and seemed eager to be tested, except in case of some of the minor children, who were so shy that a considerable amount of effort was required before a proper "rapport" was established.

The general result of the test is as follows:

Sex	Number	I. Q.			
		Median	Average	Highest	Lowest
Male	21	92	94	114	81
Female	22	97	100	143	67
Total	43	95	97	143	67

The following Table shows the distribution of the I.Q. of the total 43 children.

I.Q.	Number	Percentage
60 to 70	1	2
70 to 80	1	2
80 to 90	7	16
90 to 100	20	47
100 to 110	8	19
119 to 120	3	7
120 to 130	2	5
130 to 140	0	0
140 to 150	1	2

Fig. 1 presents the same distribution of the I.Q.

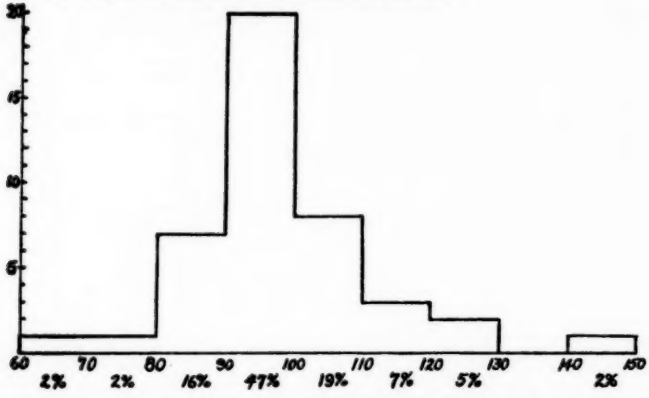

FIG. 1. Distribution of I.Q. of 43 Unselected Japanese Children, 3–12 years of age

Of this group, 24 children were in attendance at public schools, in grades ranging from I.B. to VI.B. The following Table shows the general findings of the intelligence of this group.

Sex	Number	I.Q.			
		Median	Average	Highest	Lowest
Male	13	96	96	114	81
Female	11	98	97	143	67
Total	24	96	97	143	67

The distribution of the I.Q. of these school children, which is presented graphically in Fig. 2, is as follows:

I.Q.	Number	Percentage
60 to 70	1	4
70 to 80	1	4
80 to 90	3	13
90 to 100	10	42
100 to 110	7	29
110 to 120	1	4
120 to 130	0	0
130 to 140	0	0
140 to 150	1	4

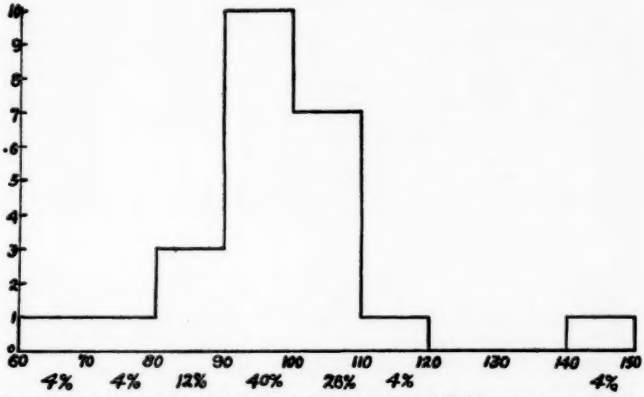

FIG. 2. Distribution of IQ. of 24 Unselected Japanese School Children, 6–12 years of age

It is interesting to note the relation of the mental ages of these children to their grades in the schools, which is shown graphically in Fig. 3. Of the total 24 cases only 8 are placed in their right grades, while 5 are too low, and 11 somewhat too high.

The coefficient of correlation between the I.Q. of these children and their teacher's estimate is 0.61, and that between I.Q. and school success is 0.47.

The average I.Q. in reference to the occupations of the parents of the children is as follows:

Professional, intellectual work; large business.....	103
Small business; skilled labor.....	82
Unskilled labor.....	90

The coefficient of correlation between the I.Q. of these children and their social status is only 0.19.

It may be of interest to note some distinguished individual cases. The one who scored the highest marks of all is a girl of 10 years and 2 months by chronological age, but who is found to be 14 years and 7 months old according to her intellectual development. Her I.Q. is 143. At the time of taking the test she was ill, but she did not at all hesitate to go through the test. No illness, according to her teacher, would prevent her from making such an attempt. She is looked upon as a very active girl, the best student in her class and the leader of her companions. Her father is manager of a branch-office of a large Japanese dry-goods store. She was born in Japan and came to this country with her father about 4 years ago. It is therefore natural that her English vocabulary is rather limited; yet she scored 30, passing Year X of the series. In the Superior Adult Test she passed Binet's Paper Cutting Test and Repeating Seven Digits Backwards Test, missing the Ingenuity Test by one. It is not surprising, therefore, to find her superior in passing the Ball and Field Test and the Healy-Fernald Form Board Puzzle in less than 10 sec. Although she was said to have skipped grades, considering her intelligence, we may say safely that she should be in the eighth grade, if not in a high school, instead of in the fifth grade as at present.

The one who scored the lowest points is a girl of 9 years and 9 months by chronological age, but who is only 6 years and 6 months old according to her mentality. Her father is an unskilled laborer, working as a farm-hand in another state and leaving his daughter under the care of another family, living in Denver at the time of this test. Her slowness was recognized by her teachers. She was still classed in the second grade. But the cause of her slowness was attributed to her undesirable environment. The present test, therefore, is very illuminating on this point. Although she scored about half the tests in Year VII and one in Year VIII, she failed miserably in those which require higher comprehension, such as the Comprehension Tests, Description of Pictures Test, and Giving Differences and Similarities Tests. There is no doubt that her slowness is due to the retarded development of her mentality.

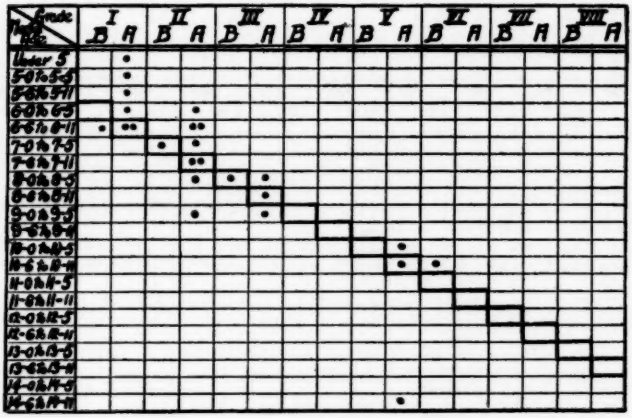

FIG. 3. Age-grade Distribution of 24 Unselected Japanese School Children

II. TRADE-NAME INFRINGEMENT[1]

By EDWARD L. CLARK

With the rapid growth of large-scale production and advertising the significance of the trade-name has greatly increased. During the years just previous to 1905 our annual national registration of trade names and trade-marks was about 5,000; in 1919 the number of registrations reached 12,218. This great increase in registrations has been marked by a still greater increase in the amount of litigation concerning trade-names and trade-marks. Whether a trade-name is enough like another to cause confusion is, in part, a psychological question. Does the purchaser remember the name or mark of the article which he is buying well enough to recognize it

[1]Abstract of a thesis submitted in partial fulfillment of the requirements for the degree of Master of Arts in Northwestern University.

the second time and avoid confusing it with a like article of similar label, wrapper, or trade-name? This problem, then, being one largely of recognition, is primarily psychological.

Experimental work relative to this problem was undertaken at Northwestern University during the winter months of 1921–22. Two methods of determining confusion were compared with each other and with the decisions of courts in a number of cases of trade-name litigation. Sophomores and upperclassmen were used as subjects. An unselected group of 18 pairs of names was taken from a large number of court records. Nine of these cases had been declared infringing and had resulted in injunctions. For the experimental work the original names were separated from the imitation names in the case of both the infringing and the non-infringing names. These 4 sets were arranged in chance orders. In the first method of experimenting, the visual method, mimeographed copies of the original names of the infringement group were presented. Following these the imitations were presented, and with them directions instructing the *Ss* to check those names which they considered as duplicates, and to indicate by a question-mark those about which they were doubtful. The same type of procedure was followed in handling the non-infringing names. In the other form of procedure, the auditory method, the *Ss* were given the same amount of time for hearing the names as the others were for reading them. Following this, instructions were given about checking names as in the visual method. While the imitation names were being read, the *Ss* recorded their reactions on sheets of paper which were handed in immediately at the end of the experiment. About one-half of the *S*'s were given the non-infringing names first, while the other half were given the infringing names first. *Ss* giving reactions to names for the first time were designated as inexperienced, while those who had before reacted to one or the other of the sets of names were designated as experienced. The following is a Table of the average number of times the different types of names were checked and questioned, arranged according to the types of *Ss* and methods. The values are expressed in percentages.

Set of Names Used	Set of Subjects	Type of Subjects	(?)	(—)	Experimental Method Used
Infringing	81	Inexper.	4.53	10.14	Visual
Infringing	93	Exper.	1.77	2.87	Visual
Non-Infrin.	86	Inexper.	3.23	7.88	Visual
Non-Infrin.	91	Exper.	3.18	5.25	Visual
Infringing	150	Inexper.	3.63	18.44	Auditory
Infringing	120	Exper.	2.22	3.98	Auditory
Non-Infrin.	120	Inexper.	2.22	10.18	Auditory
Non-Infrin.	150	Exper.	4.75	7.26	Auditory

In the case of each group, as can be seen by inspection of the Table, the percentage of names checked by experienced *Ss* was the smaller. This difference, due to the effect of experience, is much more marked in the auditory method than in the visual. The average percentage of checks of the two sets, the infringing and the non-infringing, by the visual method for the inexperienced *Ss* was 9.01, while for those who had given reactions once it was 4.06. But for the auditory method this drop was from 14.31% to 5.62%. The ratio of the change in the visual was about 11:8, while in the auditory it was about 20:8. This result apparently indicates that a knowledge of the purpose of the experiment was more capable of being used in the auditory than in the visual method. If the questioned names are considered to represent one-half as much confusion as the checked ones, the percentages then become for the visual 11.20 and 7.28 and for the auditory 15.61 and 7.36. If we express these values in the ratio used above, the two

sets of visual data are in the ratio of 12:8 and the auditory 17:8. (Since the students were told to question those names about which they were doubtful, it is fair to assume that the question-mark had one-half the value of the check. The question-mark was used to represent degrees of confusion from the just bare confusion to almost absolute confusion, so that it meant on the average just one-half as much confusion as the check.)

The results also show that there was less questioning in the auditory method even though there was more confusion. The *Ss* seemed to be more sure of their judgments when they were checking for duplication of names by the auditory method. This fact and the fact of more confusion noted above suggest that names which confuse by similarity of appearance are less likely to infringe than those which depend upon similarity of sound.

To both the infringing and the non-infringing groups were added a name and its exact duplicate. This was done to serve as a control. The control pair in the infringing group of names was somewhat shorter than the pair used with the non-infringing names, but on the average they were recognized as duplicates almost an equal number of times. The former pair was recognized 81.87% of the time and the other 85.90%. An examination of the data of those who failed to recognize the exact duplicates indicates that they checked on the average as many pairs as the other *Ss*.

In making the final grouping of data only the results from the inexperienced *Ss* were used. It was felt that the experienced *Ss* knew too much of the nature and purpose of the procedure to give results comparable with those of the inexperienced. Psychologically, the inexperienced *Ss* corresponded more closely to the buyer in the market. The order of the degree of confusion of both sets of names as determined by the visual method of procedure, when compared with the order of names as determined by the auditory method, resulted in a correlation of .86. Although the absolute amount of confusion was a little higher in the auditory method, the relative amounts were very nearly the same. The following is a Table of the confusion as determined by the combined results from the auditory and visual methods by inexperienced *Ss*. The trade-names are numbered to indicate pairs which were used in the experimenting. "N" denotes non-infringing in the opinion of the courts, while "I" signifies infringing pairs.

Order	Percentage of Confusion	Trade-Names	Order	Percentage of Confusion	Trade-Names
1 & 2	.62	N—7	10	.862	I—9
	.62	N—10	11	11.15	I—8
3	3.04	N—5	12	12.36	N—4
4	3.16	I—4	13	16.39	N—9
5	4.57	N—8	14	18.21	N—1
6	5.82	N—6	15	21.00	I—2
7	6.27	I—7	16	31.91	N—3
8	6.98	I—1	17	40.36	I—5
9	7.34	I—33	18	42.12	I—6

Assuming that the courts based their decisions entirely on the possibility of confusing the appearance, sound, or meaning of the names irrespective of their surroundings (and this was the case with several pairs of the names used), we could then by examining the data on infringing and non-infringing names determine the court's standard of the amount of confusion necessary to constitute infringement. If such an assumption were made, confusion would become dangerous under these experimental conditions somewhere between 10.39%, the average percentage of confusion of the non-infringing names, and 16.33%, the average percentage of confusion of the infringing names. If we take either of these as a standard, it is evident that in these cases the courts either based their decisions on things other than

the appearance, sound, and meaning of the names or else made several mistakes in judging the possibility of confusing the names, because neither of the percentages suggested divides the infringing from the non-infringing names. In fact, either percentage puts an equal number of infringing names on either side of the dividing line.

In speaking of percentages of confusion it must always be remembered that these percentages for these names are true only under the experimental conditions used in this work. However, the relative position of these names would not change much under other experimental conditions. Attention is called to the fact that the names do not divide themselves into confusing and non-confusing but form a continuum; many degrees of confusion are represented from the least confusing to the most confusing.

Reference

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BOOK REVIEWS

Tobacco and Mental Efficiency. By M. V. O'Shea. New York, The Macmillan Co., 1923. pp. xx, 258.

This is the first study to be issued by the Committee to Study the Tobacco Problem, which was organized in 1918 "with the object of collecting and publishing scientific data regarding tobacco and its effects, particularly physiological and economic." The statement is made that the Committee "contains both smokers and non-smokers; that the views of its individual members probably differ considerably with reference to different phases of the question, but that they are all agreed in the desire to ascertain the facts and in the willingness to accept them, whether the ultimate results may confirm or qualify the views previously held." We should expect, therefore, a study which is, in so far as is humanly possible, unprejudiced. The reader will be impressed throughout the volume with O'Shea's earnest endeavor to fulfill this obligation.

The work is divided into three parts according to the source from which the evidence is obtained. Part I contains Data derived from Observation, Introspection and Biography; Part II, Data derived from School and College Records; Part III, Data derived from the Psychological Laboratory. As the Committee has stated that its object is to collect and publish scientific data, the evidence presented challenges our attention particularly from this point of view.

A study of the "Habits of Prominent Men of the Past" (Chapter II) throws no new light upon the problem, either for or against tobacco. It does yield, however, some interesting anecdotes, such, for example, as the saying attributed to Stevenson "that if his doctor told him that smoking would kill him, he should continue to smoke since he would have to die sometime, and he was certain that nothing could bring death more pleasantly than tobacco."

The results of a questionary sent to thirty-five distinguished men in ten different fields of activity are presented in Chapter III. Striking differences of opinion are shown, and it would seem quite impossible to draw any conclusions from these observations and testimony. A scientist, for example, replies that "I do not use it and never have . . . Young men—boys—from fourteen to twenty-five, who use cigarettes, show lack of energy, hazy memory, general unreliability. More forceful, however, is the pernicious effect of tobacco in any form upon the aesthetic and moral nature . . . The habit is a vile one, its influence bad in *every* way." Another scientist states that "I was a persistent user of tobacco in all forms of smoking for more than forty years. In early life I had some prominence in athletics and have been interested in out-of-doors work since. I have not used tobacco in any form for more than two years, simply because the desire to do so has disappeared. I have not noticed at any time any change or any influence." Such statements cannot, of course, be reconciled: they are declarations influenced to a greater or less degree by the personal prejudices of the writer.

In Part II are given the data derived from a study of high school and college records. Here the attempt is made from reports of previous investigators, the replies of principals and faculties of high schools, and school and college records, to ascertain whether any relationship exists between scholarship and smoking. It is shown that smokers are inferior to non-smokers in school and college work, and that in certain fields smokers are not inferior in general intelligence to non-smokers. Considering two thou-

sand cases, a correlation of —.51 was found between smoking and scholarship, which is interpreted to mean that "smoking exerted a detrimental influence upon the scholarship of a large proportion but not all of the pupils whose records were examined." "The depressing factor is undoubtedly smoking, for the reason that the standings of smokers in the early years, before tobacco became a factor in their intellectual operations, were as high as the standings of non-smokers." The reviewer would raise a question as to the value of this evidence. A correlation of —.51, if correlation by the method of association be valid here, does nor necessarily mean that "smoking exerted a detrimental influence upon scholarship," but rather that there is a negative relationship, which should be interpreted in this case to mean that smoking is associated with or accompanied by a decrease in scholarship. There is no justification for saying that smoking in itself directly influences scholarship. It would appear more reasonable to state that some factor, or factors, possibly other than smoking, are the cause of the decrease in scholarship; and that smoking might be identified with or associated with this unknown factor upon the basis of much more substantial proof than we have at present. This seems to the reviewer the only possible conclusion which may be drawn from the evidence presented. If a pupil, for example, loses interest in school work and tends to develop certain habits, among which may be smoking, that are detrimental to success in school, then one is not privileged to draw the conclusion that smoking is the cause of the failure. Rather it is the attitude of the students towards school work—a problem which is exceedingly complex and intricate.

In the laboratory investigation, reported in Part III, apparatus was so arranged that the observer would have "all of the sensations of smoking but without the use of tobacco." "The testimonies showed that the control fulfilled its purpose completely and that the factors of interest, prejudice and suggestion were prevented from playing a disturbing rôle in measuring the effect of tobacco as a drug upon intellectual functions." "On certain days the subject would smoke actual tobacco, on other days the control pipe; but he would believe that he was always smoking tobacco." The original plan for the study called for experimental work upon high school students, and adult men and women. It was decided, however, that "it would be inadvisable to ask any woman or immature boy who had never used tobacco to undergo experiments which would require smoking."

A separate monograph is promised giving a full description of the methods and apparatus used in the laboratory investigation. We can notice, therefore, only certain general points concerning the method and results of these experiments. In the first place, the subjects are grouped as smokers and non-smokers with no reference to the actual extent of the smoking of the subjects used in these experiments or the length of time during which they have smoked. If tobacco be a drug, then it is a physiological fact that the effect of tobacco as a drug upon subjects would be greater or less according to the extent to which they had previously been subjected to it. In other words, the effect of tobacco upon a person who had never smoked would probably be quite different from its effect upon a person who had smoked for a long time. Similarly, it is possible that the effect would be different upon a person who had smoked for a year or two from that upon the person who had smoked for ten years.

In the second place, in the Summary of the Laboratory Data (Table N), where it is shown that there is a gain in some mental functions and a loss in others under the influence of tobacco, an average is taken for every subject on a large number of different tests. The units in the different tests are of unequal value and mean different things; and if a final average for every subject is to be taken, then certain tests must be given more value than others, as they represent more important mental functions. Subject 1, for instance, shows a loss of 28.40 in "Muscular Fatigue" and a gain of 22.60

in "Accuracy of Addition." The conclusion, therefore, that "it can be said that, taking a large number of individuals, tobacco will slow down and disturb the intellectual processes of the majority as shown in Table N" would rest upon the assumption that "Muscular Fatigue" and "Accuracy of Addition" are equally important functions in the mental life of the individual, contributing alike to his mental efficiency. As this assumption cannot be accepted, the results of the laboratory investigation simply show that there is a loss in some functions and a gain in others, and that it is impossible to draw any generalizations from these results.

The problem of the effect of tobacco upon mental efficiency still remains, it seems to the reviewer, in that limbo of personal opinion where one person's opinion is as good as another's. It is to be regretted that the Committee, whose definite purpose is the presentation of scientific data, has inaugurated its programme with the present volume.

SETH WAKEMAN

Cornell University

Social Psychology. By ROBERT H. GAULT. New York, Henry Holt & Co., 1923, pp. x, 336.

Under the category "social psychology," one finds a great divergence of material. The Durkheim school was principally interested in developing a defence of the group-mind concept; McDougall in presenting a rigid scheme of instincts and emotions as the core of social behavior; Wallas and Thomas in giving us a picture of personality modified largely by cultural influences; Ellwood in positing a systematic social psychology after the analogy of other systematists in psychology and sociology. Gault's book is not so ambitious as most of the previous works but is more satisfactory, in many ways, for elementary use.

Taking a sensible view of the prevalent controversy over instincts in psychology, he holds that while the concept "instinct" may be valid, much more important are the habits and "complexes" built up by conditioning, and the imagery and idea of other persons which we develop in experience. He also finds the concept "group mind" or "group consciousness" as unnecessary for his purposes as he does the system of instincts *à la* McDougall. After this preliminary survey of the field and method, he goes on to discuss the sense of social unity, social motives, intellectual and racial differences in reference to social problems, suggestion and crowd phenomena, convention, custom and morale, ending with three chapters which deal with questions of social adjustment and social progress.

The author draws upon the recent studies in mental testing, and connects them with matters of social behavior; and though sympathetic with the movement for intelligence-measurement, he is critical of the wide assumptions made about the meaning of the results therefrom. He realizes, both from the side of experimental psychology and from the side of sociology, the incompleteness of the methods and findings of mental testing. His treatment of negro intelligence, for instance, is far more fair-minded than much of the usual writing of psychologists who know the negro only through the superficial contact of the classroom and the statistics of averages and correlations.

In his chapter on social progress, Professor Gault might well have discussed the rise of the *idea* of progress itself and its psychological effect in terms of habits, sentiments and values. Instead, there is a rather too naive acceptance of progress, *i.e.*, betterment, as inevitable. There is strong hope for social advancement, social control, and the power of the environmental factors to alter present maladjustments. One or two minor criticisms may

be offered: first, too lengthy quotations are made where summarizations would suffice; secondly, the practice of omitting page-references, especially to long quotations, is undesirable.

The book is well-written, and contains well-selected concrete materials. While one wishes other phases of social psychology, such as the place of the press and propaganda in making public opinion and a discussion of the growth of new folkways and *mores*, in a book of this type, on the whole the work is welcome, and ought to appeal both to the college student and to the general reader.

Clark University KIMBALL YOUNG

Public Opinion. By WALTER LIPPMANN. New York, Harcourt, Brace & Co., 1922, pp. 427.

This is a presentation of the author's views on the factors behind the formation of public opinion. After spending three-fourths of his book in showing how public ideas and attitudes are the outcome of cleverly planned schemes of special interests, he announces a very simple plan to remedy this state of affairs by certain conscious social controls, such as bureaus of government research, which would give the people the truth and nothing but the truth. From this truth the populace would react sensibly, and all would be well.

The book is primarily of interest as an example of the use of one psychology, based on the analysis of instinctive, unconscious trends making for an irrational type of social behavior, as witnessed in the creation of public opinion, and then, at the close of the volume, the use of another psychology, bearing the tinge of eighteenth-century rationalism, *plus* that of recent social idealism, in projecting an utopia of a perfectly adjusted human nature and social order.

The concrete material is excellent, and the book is well-written. Mr. Lippmann has had some years of newspaper experience, and knows whereof he speaks on the practical side of journalism.

Clark University KIMBALL YOUNG

Studies in Mental Deviation. By S. D. PORTEUS. Vineland, N. J., The Training School, Department of Research Publication, No. 24. October, 1922, pp. xi., 276.

This volume is made up principally of collected papers which report researches completed under Dr. Porteus' direction. In reading these papers one feels that the writer has an intense, human sympathy for the problem of the mentally subnormal, a keen appreciation of the subtlety of the sociological factors making for defectiveness and delinquency, and a trained mind in devising novel ways of attacking the important social and racial problem of the defective through the aid of psychology. In reference, for instance, to the oft-repeated assertion that "every defective is a potential criminal," the author points out that, of the large number of cases he has studied, only "13% show delinquent tendencies," and further that "the really great menaces to society are the dull normal delinquent and the psychopathi groups" (pp. 24, 25).

After giving a brief survey of the aims, methods and materials of his work, he goes on to present his results. There is a significant chapter dealing with new anthropometric studies on brain-capacity and physical growth. There are two valuable chapters on his own maze-tests, and an excellent chapter on a social rating-scale for personality-traits, the results of which

show the inadequacy of both the Binet scale and his own maze-test, and by implication of all similar tests, in diagnosing the social worth of an individual. His standpoint on the Binet scale in Chapter VII ought to be read and reread by every worker in the field of testing. Quoting a previous article of his own he writes: "The criticism that may justly be founded on this analysis is that the tests are far too 'literary.' They favor the glib-tongued, quick-thinking child, the child who has had a good educational environment, the child who memorizes readily and to whom expression is easy, and who, therefore, shows good scholastic promise" (p. 188). At the same time, they penalize, as he shows, the shy child, the child of slower, more deliberate thinking, and all those whose chief traits run to the vocational, manual sort. At the close of this chapter he indicates the inaccuracy of attempting classification of feeblemindedness on the basis of the Binet scale alone.

The concluding chapters present material on the use of educational tests with defectives, on performance-tests, and on a new industrial rating scale which he has devised.

The book is readable, and contains a solid array of concrete data of value to the psychologist, sociologist and social worker.

Clark University KIMBALL YOUNG

The Psychology of Social Reconstruction. By G. T. W. PATRICK. Boston and New York, Houghton Mifflin Co., 1920, pp. xii, 274.

The author weaves together in this book an interpretation of contemporary history in the light of the psychology of instinct, a criticism of current programmes of social reconstruction, and some positive suggestions of his own as to proper methods of social reform.

Many recent events—beginning with the dancing craze and ending with the World War—indicate that man's instinctive nature, after a period of "getting civilized too rapidly," is now reasserting itself. So quickly has our social environment changed, while our innate dispositions have remained unaltered, that under present conditions some of our most insistent tendencies find themselves blocked, and the lack of an instinctive basis threatens society with disintegration. If this diagnosis is correct, any attempt at social reconstruction must take the psychology of instinct into account. The popular schemes of reconstruction, however, all derive from a one-sided theory of pleasure-economy.

Education, eugenic regulation, and the application of science to social problems are the remedies that the author prescribes for the malady which he would characterize as a "temporary social fatigue of the higher brain centers."

S. FELDMAN

Cornell University

Unconscious Memory: with an Introduction by Professor Hartog. By SAMUEL BUTLER. New York, E. P. Dutton & Co. [1923.] pp. xxxix., 186. $3.00.

Butler's "Unconscious Memory" was published in 1880, and reprinted with a Note by R. A. Streatfeild and an Introduction by the professor of zoology in University College, Cork, in 1910. The present edition is a reprint of that of 1910, with the addition to the Introduction of a one-page postcript (dated 1920), in which Professor Hartog brings together some of the chief references to Butler's biological work. The book has its place, of course, in the series of Butler's writings; but it seems a pity to recall old controversies; and those interested have long ago made up their minds about Butler and Darwin.

The main interest to the psychologist is the translation of Hering's lecture on memory. Butler's translation of Hering, and for that matter his translation of von Hartmann also, cannot be called accurate; but it is written in admirable English, and might be made the basis of an exceptionally good rendering.

The book would have benefited by editing. Le Dantec and Semon are no longer living; Baldwin is not now professor of psychology at Princeton; and the remark "The lecture is published by Karl Gerold's Sohn, Vienna" is hardly an adequate bibliography of Hering's address. Worst of all, the book has no index.

E. B. T.

Discours de la methode, by DESCARTES. Cambridge Plain Texts: Cambridge University Press, 1923, pp. vi., 74.

In 1899 the Open Court Publishing Co. issued, for twenty-five cents, an edition of Descartes' *Discourse on Method* in Veitch's translation. John Veitch was a professor of logic and rhetoric in the University of Glasgow, who published in 1850–1853 translations of the Method, the Meditations, and selections from the Principles, which combined in a single book went through many editions. The Chicago reprint of the Method has a portrait, a 4-page preface, two pages of 'Descartes literature,' and a two-page index. The portrait and the index are worth while; the bibliography is a failure.

The Cambridge University Press now gives us, for fifteen pence, a reprint of the Method in French, with a two-page introductory note. This introduction, which is partly biographical, is better than the Open Court preface. The French text has a single foot-note, which informs us that "*poêle* = room with a stove;" a fact that we might, perhaps, if only by the aid of *stove* and *Stube*, have arrived at for ourselves. The Cambridge editor, on the other hand, fails to explain the *ce volume* of pp. 27, 63; Veitch properly reminds us that the first edition of 1637 contained, besides the Method, the Dioptrics, the Meteorics and the Geometry.

The right introduction to such reprints is, surely, a combined biography and bibliography, set out in the form, not of an essay, but of a chronological table. The reader—the kind of reader that the books attract—would welcome this summary orientation; undocumented references to St. Augustine and St. Anselm will leave him blank or irritated.

E. B. T.

NOTES

The Leipzig Congress of Psychology

The eighth biennial congress of the *Gesellschaft für experimentelle Psychologie* was held in the Psychological Institute of the University of Leipzig, April 17th-20th. Professor G. E. Müller, the president of the *Gesellschaft*, served as chairman. There were approximately nine hundred registered visitors, of which number sixty came from countries other than Germany or Austria. Fifty papers were read in the course of the congress, divided between two simultaneous sessions. One session dealt exclusively with problems of general psychology, and the other with the various specialized branches. The papers might be classified according to their content as follows: *Sammelreferat*, dealing with the *Strukturbegriff* in German psychology, 1; *Sammelreferate*, dealing with the problem of personality, the central theme of the congress, 3; general papers dealing with psychological theory and methods, 15; reports on experimental work, 13; applied psychology, 7; social psychology, 3; animal psychology, 3; child psychology, 3; psychopathology, 2. Six of the papers were presented by foreign psychologists, viz., Spearman (London), Grünbaum (Amsterdam), Gudaitis (Kowno), Juhász and Nagy (Budapest), and Sneersohn (Moscow).

Professor Krueger (Leipzig) opened the congress with a *Sammelreferat* dealing historically and critically with the concept of psychical structure, the central doctrine of the *Gestalt*-psychology.[1] The concept, said Krueger, has been developed within the past decade simultaneously from four directions, the psychologists of each direction working quite independently of the others. The time is now ripe for a comparison and conciliation of these various expressions. The four sources to which Krueger referred the doctrine may be briefly characterized.

(1) *The approach through perception.* Wertheimer in 1912, in his report on the perception of movement as a centrally produced conscious content (the *Phi-Bewegung*), proposed the hypothesis that for the *Auffassung* of phenomenal movement it is not the excitation process in the excited cells themselves, nor the sum of the single excitations, which is essential. The principal rôle falls to characteristic cross-processes and total processes, which result as specific wholes (in larger areas) from the excita-

[1]It should be kept in mind that the *Strukturbegriff* in modern German psychology differs significantly from the concept of structure in American psychology. Whereas in American structural psychology the composition of experience at a given moment is expressed in terms of the 'mental elements,'—such, for example, as sensations, images, and affections,—the very essence of the German *Strukturbegriff* is to deny the existence in experience of these or, indeed, of any mental elements. The mental structure, or *Gestalt*, is in every case something more than a summation of elements. Each experience has a uniqueness, a totality and an indivisibility which are not referable in any sense to 'constituents'. American structural psychology, furthermore, assumes a close correspondence between stimulus and sensation, while the German doctrine emphasizes that the state of the entire nervous system is a factor of great importance in the stimulus-*Gestalt* relationship. The *Gestalt* is in no sense a simple or direct function of the stimulus. A sensation, the alleged function of the stimulus, never exists in experience; it is a psychological abstraction.

It will avoid ambiguity if we designate the German structuralism as *Gestalt*-psychology, but it will be necessary to keep in mind that the original conception of *Gestalt* relates rather narrowly to the sphere of perception, and that the doctrine of the *Struktur* has been extended in other directions.

tion of single cells (the *Einfallstelle*).[2] Sensory data are always to some degree *'gestaltet'*, in such a way that the resultant content is possessed of concrete *Ganzeigenschaften*, of *Ganztendenzen*, and of an 'inner' *Gesetzlichkeit* of its own. Working in the same direction as Wertheimer we find Koehler who concerns himself with the theory as it relates to the physical and physiological correlates of perception. According to Koehler *Gestalten* are those psychical states and processes whose characteristic qualities and effects are not composed of the qualities and effects of their so-called parts. Koehler undertakes to establish the *Reiz-Erlebnis* relation which exists between the physical systems in the outer world which he believes possess *Gestalt*-characteristics and the phenomenal or psychical *Gestalten*.[3] Proceeding from this general position of Wertheimer and Koehler is a variety of contemporary research which aims to formulate in terms of *Gestalt*-psychology the laws of limen, phenomenal movement, differential threshold, contrast phenomena, fusion, optical illusion, etc.

(2) *The approach through Geisteswissenschaft.* Dilthey has been influential in directing much psychological thinking away from the explaining and analyzing method, and in establishing a psychology in which the mental *Lebenszusammenhänge* are regarded as indivisible and inseparable functions of the entire personality. The mental life as a whole is regarded as a teleological structure. The most that this new psychology can hope for, says Dilthey, is to establish a *Typenlehre*, or a systematic exposition of the mental life of a typical man.[4] E. Spranger, Dilthey's disciple, defines the psychological task not as a search for the ultimately distinguishable elements, but as a search within a higher level of concepts for the distinguishing characteristics of the *Gesamtsituation*. The practical consequence of Spranger's approach is also a *Typenlehre*.[5]

(3) *The genetic approach.* Krueger, starting with v. Ehrenfels' conception of *Gestaltqualität*—which ascribes to tonal and optical perceptions an independent quality in addition to the sum of the single sensory excitations: *e.g.*, the *Gestalt*-quality in a chord of music—maintains that these *Gestaltqualitäten* in reality represent the essence of the perception, and exist *in place of* any constituent or partial experiences. In every case the 'psychic totality' is a unity quite independent of its parts. Each experience is possessed of a peculiar *Komplexqualität* of its own. Splitting this *Komplexqualität* in any way into elements or atoms destroys the very essence of the experience. In primitive man and in childhood we find that consciousness is predominantly a matter of an undifferentiated, fused, and complex content. Krueger emphasizes strongly the merit of studying the *Gestalt*-principle genetically.[6]

(4) *The approach through Personalismus.* As a supplement to his psychology of individual differences, Stern has stressed of late years the unity of the personality as an indivisible object for psychological study. The mental composition of the individual, the elementary psychical processes, habits, traits, and unit characteristics are by no means the extent of the psychological problem. The entire synthetic side of man's nature, without consideration of which any isolated process or trait would be meaningless, must be considered in all its uniqueness and indivisibility.

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[2] *Cf.* M. Wertheimer, Experimentelle 'Studien über das Sehen von Bewegung *Zsch. f. Psychol.*, 1912, 61. Also Untersuchungen zur Lehre von der Gestalt, *Psychol. Forschung*, 1922.

[3] *Cf.* W. Koehler, *Die physischen Gestalten in Ruhe und im stationären Zustand, eine naturphilosophische Untersuchung*, 1920.

[4] *Cf.* W. Dilthey, *Die Typen der Weltanschauung, Sammelband: Weltanschauung, Philosophie, und Religion*, 1911.

[5] *Cf.* E. Spranger, *Lebensformen*, 1922.

[6] *Cf.* F. Krueger, *Ueber Entwicklungspsychologie; ihre sachliche und geschichtliche Notwendigkeit*, 1915.

Man is *unitas multiplex*. The unity does not consist merely in the addition or summation of traits, but in the harmonization of all psychical processes within the individual (viz., his experience, his acts, his dispositions, and his Ego) into an active purpose. More fundamental than differential psychology, by far, is the problem of the nature, the activity, and the unity of the *total personality*.[7]

The *Sammelreferat* by Krueger may be taken as representative of the Congress as a whole—a unification of the various expressions of the *Strukturbegriff*. There can be no question that this energetic movement is the prevailing influence in contemporary German psychological thinking. The majority of the papers before the Congress assumed the axioms of *Gestalt*-psychology as their point of departure. The theme set for the Congress, the psychological problem of personality, itself indicates the current emphasis upon an undivided, complex psychological object. Selz (Bonn) and Sommer (Giessen) presented *Referate* concerning methods for the determination of personality types, and Peters (Mannheim) reviewed recent work concerning the influence of inheritance upon personality.

Among the specific researches relating to the general *Gestalt*-movement should be mentioned the report of Jaensch (Marburg) concerning the *Eidetiker*-disposition. Jaensch asserts that the primary after-image is the lowest stage in a continuous series of memory-levels. Between this stage and the *Vorstellungsbild* he claims the existence of the *Anschauungsbild*. The *Anschauungsbild* represents a retention of the original undifferentiated perception. The individual whose images are primarily of this type (the so-called *Eidetiker*) possesses the ability to reproduce with astonishing detail the original *Vorlage*, even after a considerable time has elapsed. A greater or less degree of the *eidetische Anlage* is to be found in all children, and frequently (if not always) in poets and artists. Jaensch proposes that a comparative psychology of races might well begin with a study of such a disposition. In this connection he offered the results of his tests for the *eidetische Anlage* in German and in French school-children in the city of Mainz. A further application of the work is in the field of pedagogical psychology where a new understanding is needed of the *Erlebniswelt* of the child. The testimony of the child's *Anschauungsbilder* shows that his world of experience is in no sense that of the adult, and that in general there exists for him a much 'rawer' relation between the *Wahrnehmungsbild* and the *Vorstellungsbild*. The *Anschauungswelt* in its structure is similar to the *Vorstellungswelt*, and yet is itself an exact copy of the *Wahrnehmungswelt;* hence for the *Eidetiker* the image and the perception bear a point-for-point correspondence.

Volkelt (Leipzig) reported his studies of the primitive *Komplexqualitäten* in children's drawing. It is characteristic that a young child, drawing from a model, reproduces not what he actually sees or what he knows to be the nature of the model (*e.g.*, a cube), but an elementary combination of the two, a complex quality; that is to say, he draws the essence of the thing in its totality as it *affects* him.

Katz (Rostock) reported upon a study of intelligence and character differences among animals. Working with chickens, partly with his former methods and partly with the tests devised by Koehler for apes, he arranged rank orders among the animals in regard to intelligence and certain traits of character, notably leadership. The character traits represented individual differences which were definitely not related to intelligence. Ettlinger (Münster) contributed a study concerning the use of tools by animals.

The papers dealing with applied psychology presented research pertaining chiefly to problems of industry and vocational selection. The

[7] W. Stern, *Die Psychologie und der Personalismus*, 1917. Also *Die menschliche Persönlichkeit*, 3d. ed., 1923.

Gruppe für angewandte Psychologie holds its congress every two years, alternating with the congresses of the *Gesellschaft für experimentalle Psychologie*. This arrangement accordingly reduces in the larger congress the number of papers on applied subjects.

Among the remaining papers of the Congress were reports by psychologists whose general lines of research are fairly well known in America. Many of these papers contained a reshaping of old concepts to the prevalent *Gestalt*-psychology. Very little of the classical German psychology remains in active use. It was characteristic of the Congress that in none of the lectures or discussions was reference made to the name or the teachings of Wundt!

The material of these various papers will appear in due time in the usual periodicals. (Whether a *Bericht* of the Congress as a whole will be issued this year has not yet been decided.) The present report aims to represent the general *Stimmung* of the Congress, and to point out the newest and most characteristic lines of research. The American psychologist who will familiarize himself well with these researches and with the underlying concepts will find them invariably suggestive, and often challenging.

Hamburg GORDON ALLPORT

VISUAL ACUITY AND ILLUMINATION

In a recent paper,[1] Ferree and Rand show that visual acuity increases with increase in the intensity of illumination and that this effect is more pronounced in eyes slightly defective as to refraction than it is in normal eyes. They state further, that "the explanation of the effect of increase of intensity of light on the power to see clearly is to be found, of course, in terms of the resolving power of the retina, not of the refracting media."[2] The correctness of such an explanation may be questioned. Does the resolving power of the refracting media, that is, their ability to form optical images upon the retina, remain constant under changes in illumination? If it does, the resolving power of the retina must be the variable and the explanation is correct. But if the refracting system does not remain invariant under changes in illumination, the explanation given by Ferree and Rand is inadequate to the facts.

The resolving power of the refracting media of the eye depends in part upon the size of the pupil. The more peripheral zones of the lens-system suffer from errors of refraction to a much greater degree than the central.[3] Closure of the iris increases the definition of the optical image thrown upon the retina, just as the use of a diaphragm in the photographic lens increases the definition of the image thrown upon the sensitive film, that is, increases the resolving power of the photographic lens. Moreover, the size of the pupil of the eye varies with illumination, becoming smaller as the intensity of the light increases.[4] This means, of course, that as the intensity of illumination increases the errors of refraction cause less diffusion of the image because the iris cuts off from the refracting system its peripheral zones. The resolving power of the refracting media of the eye thus increases with increased illumination.

We see, then, that an explanation of the effect found by Ferree and Rand in terms of the resolving power of the retina does not do justice to all the facts of ocular response. Change in the resolving power of the refracting system is at least one factor causing acuity to vary with illumination. Whether or not change in retinal sensitivity is a further factor in producing

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[1] C. E. Ferree and G. Rand, this JOURNAL, 34, 1923, 244 ff. [2] *Ibid.*, 244.

[3] E. H. Starling, *Principles of Human Physiology*, 1920, 507; W. H. Howells, *A Text-Book of Physiology*, 1915, 319.

[4] E. H. Starling, *op. cit.*, 507f.; W. H. Howell, *op. cit.*, 327.

Although adaptation to low illumination deceases the amount of constriction of the pupil, it does not eliminate it.

variations of acuity can be determined only by observations in which the size of the pupil is held constant, either by means of an artificial pupil or by paralyzing the iris with atropine or some similar drug.

University of Pittsburgh GILBERT J. RICH

OBSERVATIONS ON TAKING PEYOTE

Since writing my paper on Peyote (this JOURNAL, 34, 1923, 267–270) my attention has been called by Mr. Carl Boyer of Philadelphia to a previous article by Havelock Ellis. This article, entitled "Mescal, A Study of a Divine Plant," appeared in the *Popular Science Monthly* (61, 1902, 52–71). Ellis reports in full his own experiences and those of another subject on taking Peyote and also indicates the results of several other subjects. In general Ellis is interested in the visual phenomena, which are generally obtained and which I failed to get to any extent. But he also indicates other results which seemed to be more or less unique in my own case and which have not elsewhere appeared in the literature to my knowledge. Ellis notes the clearness and duration of after-images, and the magnification and increase in clearness of objects in the periphery of the visual field, and insists (as I found) that there is a general "hyperaesthesis to all sensory impressions." For the other subject whose results are reported both time and space were upset; for Ellis himself time was not affected. But Ellis reports an "inability to fix attention long," which corresponds closely to my own findings. Indeed, I attempted to analyse the effects of the drug in my own case in terms of a very vivid but very rapidly changing focus of attention.

SAMUEL W. FERNBERGER

University of Pennsylvania

THE EXPRESSION OF SIMPLE FEELING

In a recent critical Note I called attention to a quotation from one of our Cornell Minor Studies, a quotation so changed from its original, by omission, that I feared it might be misleading.[1] Professor Bentley now charges me with a misstep from logic of the same kind as that which I noted in the quotation to which I was referring: if that quotation was misleading by mutilation, my critical comment is misleading by omission of context. "Titchener should have been warned that Young's 'therefore' (in the quotation 'It is impossible, therefore, to accept Corwin's interpretation . . . ') refers backward to the very qualification which Titchener seeks." The next preceding paragraph should thus have put me right.[2]

I still believe that my criticism was justified. But the reader shall decide for himself; I print below the relevant passages. I give (*A*) the paragraph in question; (*B*) the immediately following sentence, containing the mutilated quotation; and (*C*) the quotation restored to its proper form.[3]

(*A*) There can be no doubt that "seeking" movements *may be* associated with P. Corwin's experiment proves the point; but the conditions for such an association involve two factors: (*a*) a weak or receding stimulus, or some other unsatisfactory element in the conditions—a need, irritant, incentive, motive—which makes it necessary for *S* to seek or pursue if he is to react adequately and to maintain conditions under (*b*) stimulation which is normally P. The factors which condition seeking movements are obviously neither necessary nor favorable to the existence of P feeling. The neces-

[1] This JOURNAL, 34, 1923, 149.

[2] *Psych. Rev.*, 30, 1923, 326 f.

[3] (*A*) and (*B*) are from this JOURNAL, 33, 1922, 522; (*C*) is from 32, 1921, 570.

sity for seeking reduces the intensity of P obtainable and the reduction may go to I or even to U; extreme cases of seeking are U rather than P. If seeking movements express anything, that is an incentive, need or irritant rather than simple feeling.

(*B*) It is impossible, therefore, to accept Corwin's interpretation that "the direct response of the organism to P is . . . a definite activity of pursuit or of tendencies to pursuit."

(*C*) The direct response of the organism to P is, as stated above, either relaxation *with a certain degree of expansion*, if the stimulus is weak or stationary; or, if the stimulus is intense, and if the source of the P is withdrawn, a definite actifity of pursuit or of tendencies to pursuit.—

I have said that (*C*) is significantly different from the quotation of (*B*). Bentley, if I understand him, replies in effect: 'Granted, so far as it goes! but you have missed the poit that (*B*) is made to follow logically from (*A*), and that (*A*) contains the very qualification whose omission from the quotation in (*B*) you criticise.' I reply, in my turn, that (*B*) expresses, not agreement or acceptance, not even qualified agreement or acceptance, but flat disagreement,—and, to repeat my former criticism, disagreement with something that is significantly different from what Corwin wrote.

E. B. T.

Alcohol, Tobacco, Tea: Postscript

After the MS. of my article had gone to press, I received the recently published book on "Tobacco and Mental Efficiency" by M. V. O'Shea. Not a few of the statements of this book invite criticism.

Is it (scientifically) legitimate to talk of the influence of tobacco—a material substance, a drug—on "meditation, reflection, judgment, alertness, ambition and the creative activity," terms which fit well in any "faculty psychology," but which nobody can conceive of in anatomical-physiological terminology?

Here is a sample of the treatment given the tobacco problem in this book (p. 115) with reference to literary men and women who use tobacco: "He has now associated smoking and creative activity together, and the creative faculties will not function unless the pipe or cigar or cigarette is in hand or in mouth." Is that not old-fashioned phrenology? *Does not the creative faculty declare a sympathetic strike for the benefit of the smoking faculty which has been deprived of its weed?* The quotation is given as "a probable explanation" of the help which some people derive from tobacco. What is the meaning of "an explanation" among men of science? To me the above "probable explanation" is fiction.

Where proposing scientific problems differs so profoundly in method, a fruitful exchange of opinions seems precluded. In another instance, however, an agreement can be reached, since I may have failed to understand a description given by the author of the book. A certain "control experiment" was made at the author's request by Dr. Hull. "The function of the control was to keep all the factors in the experiment uniform except the one factor of the drug effect of tobacco." The reader of my paper recalls that I produced the drug-effect by filling the air with tobacco-smoke. In Dr. Hull's "control" S, blind-folded, had in his mouth an electrically heated, but tobaccoless pipe; and E close to him smoked a pipe filled with tobacco "so that the subject smelled the tobacco which he thought he was smoking, but which was smoked by the experimenter" (p. 181). I fail to comprehend how in this "control" S, who had to breathe the smoky air, *was protected against getting the drug effect of tobacco.*

I have some further experimental data on drugs which I intend to publish at a convenient time in the future.

University of Missouri Max F. Meyer

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